VOL. XXVIII OCTOBER, 1918 NO. 4

THE MONIST

WHEELER'S HUNDREDTH-CENTURY PHILOSOPHY.

OCCASIONALLY a writer, unversed in academic formulas, arises from the rank and file of men to challenge the truth of our most cherished creeds and combat tenets that bear the sanction of armies of sages. Jacob Boehme, the "Shoemaker Philosopher," despite his lack of scholastic training, deemed his inspiration commanding and potent. Flashes of light that to him could have no other source than the *Urgrund* of Eternal Verities broke in upon his soul. Their illuminations must be reported to the world or his charisma would fail, and God's purposes be thwarted.

Such, too, though with a different attitude toward the traditional God-conception, was the propagandic spirit of the late Dr. Charles K. Wheeler, author of the *Hundredth Century Philosophy* series.[1] His earlier works, at least, contain a thoughtful message to the philosophic investigator. The problems presented were mainly fundamental, and their treatment commendably original. His ontology is radically at variance with prevailing systems, and in his psychology many startling propositions appear.

The author's unfortunate idiosyncrasies of expression must prove a barrier to popularity until some patient interpreter recasts the English into forms more conventional,

[1] Published by Mrs. C. A. B. Wheeler, 330 Massachusetts Ave., Lexington, Mass.

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and more readily intelligible. The fault lies not in a novel nomenclature, but in the use of unwonted and unwarranted grammatical forms, compelling the suggestion that some type of aphasia worked havoc amid the subtle play and trenchant piercings of an intellect otherwise endowed with analytic powers of a high order. It is to be regretted also that the author indulged so freely in an undignified pasquinade in his final volume: *A Critique of Pure Kant.*

But his work deserves attention for its unique methods of attack upon the Kantian metaphysics, whatever value may be assigned to his interpretation of the tenets of the Königsberg sage. The Ontological Realism which our author submits as a substitute for all academic philosophies based upon idealistic epistemologies as presented by the triumvirate, Kant, Hegel, and Schopenhauer, merits investigation also, as it presents many interesting features.

But despite the iconoclasm and revolutionary boldness of Wheeler's intellectual ventures, his work has, as yet, won but little attention from philosophic inquirers. This neglect results, no doubt, in large measure from what the *Westminster Review* terms "a style excruciatingly tortuous and obscure." Nevertheless the *Review* finds "much that deserves serious attention, and which it would be difficult to refute." "The author," says the reviewer, referring to Wheeler's *Critique of Pure Kant,* "often speaks out—we wish we could say always in plain English—what many students must have thought."

In common with all reformers and idol-breakers, Wheeler regarded his affirmations final and abiding. But finding in his latter days that the world paid little heed to his revelations, he exclaimed, with the pathos of a hope deferred, "Some far day I will be rediscovered as was the revealer of Mendle's Law." But his importunate desire for recognition seems doomed, at least for a time.

Boston papers, recording his death, quoted a line origin-

ally written on the death of Bayard Taylor, "Dead he lay amid his books," for in his casket was placed a set of the volumes he had written, and pine-branches were strewn upon his coffin "as fitting emblem of a long and fruitful life," for Wheeler's learning had long been recognized, and his work as a lecturer had often given inspiration to Boston's intellectual circles. Besides, he had for years served the city officially as Assistant Physician. His story is that of many a passionately earnest seeker after Truth, and his confidence in his conclusions is echoed in the somewhat presumptious, or, perhaps, despairing title that identified his speculative volumes: *The Hundredth Century Philosophy.*

But let us learn somewhat of the thinker who assigned to the basis of all academic systems of philosophy no more substantial a value than that of an evolutionary and transient product, and who denied the primacy and transcendency of mind and consciousness, designating them as degenerate forms of a Primordial Mental, even as is matter also. Let us consider the dialectic processes by which our author essays to prove the invalidity of our most intimate and persistent concept, that of our being conscious and self-conscious egos. Let us learn why he refuses to subsume sensibility and memory under the category of mental attributes, and how he argues that all conviction of truth, whether in mathematics or in morals, reaches back into sensuous experience, and even into the mechanicality of that experience, for its source. And let us trace briefly the arguments through which are challenged all ontologies that deny the existence of an outside world absolute and identify all its manifestations with mental experience. For these and other theorems quite as startling constitute the contributions Wheeler has offered to the thinkers of the world.

The most fundamental of his postulates is that mind and consciousness are not ultimate entities, but the after-

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math of a Primordial Mental of wholly different character, and of which matter is, perhaps, a basic manifestation. This Primordial Mental, and not its by-product, human reason, is the only "substance" that can properly serve as a basis of knowledge, even though that reason be assumed to find its validity in transcendental realms, as the root of Yggdrasil was fabled to pierce the mystic kingdom of Hela, and draw its sustenance thence.

While products of no state of the physical of which we yet have any knowledge, mind and consciousness will nevertheless be explained eventually in terms interchangeable with definitions of the material; for matter, being of a nature self-existent, must be esteemed of a higher grade of being than are the dependent existents, life, mind, and consciousness, in the media of which thought revels with such assumed superiority.

"Whether consciousness is limited to the brain and nervous tissue of animal life, or whether not, no one knows, or is ever likely to know, with the probabilities, however, that it is not. But consciousness, whether obtaining outside animal life, or only inside, results from an event logically and historically anticipating it, and is, therefore, in any case, itself nothing aboriginal, nothing primary, but is an evolutional product lamentably restricted in function." But the Primordial Mental is both soul and body of the universe, for matter is the mental *in statu quo,* perhaps a permanent, normal manifestation of the Absolute Reality, as indeed was posited millenniums ago by the Vedantists in their creeds of Prana.

Upon the modes of the activity of this substantive entity depends the character of all human mentality. These protean manifestations, however, present no clue to the ultimate nature of the substratum. A ball or cube whirled rapidly yields to the eye the phenomenon of a stable circle from which no inference could be adduced of the revolving

object producing the delusion. Matter can be cognized only through, or in the phases of, its activities, and our interpretations of these phenomena are perhaps as erroneous as is the sense of the ring produced by the revolving ball. Nevertheless, these interpretations serve our uses; for the whole nexus of our interrelations with the manifest universe involves the application of these interpretations.

The ultimate is, therefore, to us only the activity of the Absolute. Mind itself is an activity. Introspection reveals nothing more than the acts of perceiving, feeling, conceiving, etc. Of what these acts are the manifestations we possess no faculties for discerning. "Can a thing in motion," asks Wheeler, "be the motion of that in motion? Can the ball itself be what is only the motion of the ball? Is there identity of nature between the clock which goes and the going of the clock?. . . . How then shall that which is active be itself mind, if what is its activity be mind?"

We cannot reason back from the finite to the unconditioned infinite forms of mind. Analogies are conceivable and inviting, but not proofs. "It is absolutely unthinkable," says Wheeler, "that the infinite shrunken to finite should not suffer a jolt in the transition that constitutionally must radically alter it as well as circumscribe it."

Wheeler's demonstration of his postulate that conscious mind is but a by-product is presented in the following quotation: "Modern science informs us that visual light is due to the impact of ether-vibrations on eye and brain. Vibrations are something mechanical. So that visual light is due to the impact of something mechanical on something. But science, again, informs us that to a change, itself something mechanical, of the wave-lengths and of the frequency of their impact (also something mechanical) is due a change of consciousness, that is to say, of its content, from a consciousness, say of red, to a consciousness of blue, green, or other color. But now, if to a change of the mechanical

making the impact, is due a change of consciousness, that is, of its content, then, *prima facie,* to the mechanical minus the change, that is, to the mechanical *in statu quo,* is due content of consciousness at all, since we know the latter only as with content as we know reflection in a mirror only as with objects before it for reflection. But now again, consciousness due to the impact of something mechanical on something, is consciousness preceded, of course, by that something making the impact and on which the impact is made, and which, as preceding, is itself the thing of primacy and transcendency, and not consciousness that is such, which itself then obtains only as a development, and as only a development then only as an aftermath."

The most primary movement of the Absolute Reality of which we have any knowledge, says Wheeler, is that centrifugal gyre by reason of which solar and stellar revolutions result. The centripetal, or return-stroke of this movement, with the ensuing impact upon itself, engenders life, mind, and consciousness.[2] These products are in-

[2] "I have sought to make evident that consciousness springs of the impact of the Absolute Reality on *itself.* Indeed, if I might be indulged in a certain distinction between, say, Absolute Being and Existent Being, that I may express an idea, then I would say that the Absolute Reality's most primary move out of Absolute Being into Existent Being, or what is recognized as the manifest universe, is one of projection of itself in the centrifugal and return upon itself in the centripetal, coincident with which movement, worlds revolve around worlds and suns around suns; while synchronous with the return-stroke of that movement and impact upon itself, obtain in ascending order, existence, life, mind, consciousness. That is, everything within our knowledge takes its rise primarily in the *mechanical* consciousness.

"Let me be understood about this. Every physician makes the distinction between the predisposing cause of disease, and the exciting cause. Now, in the impact of the Absolute Reality on itself, it is the mechanical of that Reality that, as exciting cause, making impact on what, as the predisposing cause, is not the mechanical of that Reality, that gives rise to the events of which I spoke, and to the event of consciousness, with the rest. Clearly, if this be so, then consciousness is not the primary thing, but the Absolute Reality, itself consciousless. With it is lodged, latently, both sorts of cause. Water freezes at 32°; but the temperature can be carried lower without freezing if the water is not disturbed. But touch it with a stick, and instantly it congeals. The water, however, has no power to disturb itself; with it is not lodged the exciting cause but only the predisposing cause. But with the Absolute Reality is lodged the power and impulse to be its own exciting cause, that is, power to make impact on itself, which is its exciting cause to consciousness..... Moreover, if any well-nigh universal consciousness as above implied really obtain, it must yet be an aftermath of Existent Being, and no more conscious

capable of perceptual cognoscence save through the efficiencies of stimuli imparted by an external world, the intermediary and interpreting medium being neural and cephalic substance in living organisms, and without which intercommunication were impossible. But the Primordial Mental possesses within itself the capacity for consciousness unassisted from without. The mental of Kant and of all academic theorists is wholly of the former type.

The brain, in its functionary capacity, is not the demiurge of consciousness, nor can it properly be said to yield the content of consciousness: it merely determines its content, such as it is. A content cannot be conceived as conscious, much less as self-conscious. Since consciousness can know only its content, that supposititious self back of consciousness, in order to become known, must become content, even though consciousness be reckoned a function or attribute of that assumed entity.[3]

Furthermore, consciousness, by the same token, cannot cognize itself. Some image, evoked by imagination, must pose as a symbol of it. But this symbol is neither conscious nor self-conscious: it is simply consciousness, Dr. Wheeler declares, "holding a lifeless picture of itself upon its lap." To the exploitation of this idea our author has devoted an

subjectively of anything going on within Existent Being than are we conscious of the bones, nerves, blood-corpuscles, and goings-on within our physical bodies."
—From a personal letter.

[3] This idea is presented in the following quotation, which also affords an example of the verbal spirality and indirectness with which Wheeler worms his way through a thought: "There can be no self-consciousness of a thing not, itself, conscious. But, if there can be no self-consciousness of a thing not itself, conscious, or until it is conscious, then, once it is conscious, there can be no self-consciousness but of what was in the consciousness of the thing simply conscious. And, as what was in the consciousness of the latter or the conscious self, the self not consciousness itself, yet, however, that something having consciousness or being conscious, was nothing of consciousness of that self (or it would be already self-conscious) nothing of that self, even, much less of that self as such, than in the any self-consciousness as of such conscious self, there could be nothing either of consciousness of that self as such, nor, even of what was that self merely. That is, the self as something distinct from consciousness, not being in the consciousness of the conscious self, would be utterly beyond the reach of any act of self-consciousness."
—*Autobiography of the I or Ego*, pp. 19-20.

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ingenious volume entitled: *The Autobiography of the I or Ego.* This work was editorially reviewed some months ago in *The Open Court.* It is an iconoclastic attack, as stated above, upon our most intimate and persistent concept, that of our being self-conscious egos.

Actors, simulating others, and certain types of insane persons, losing the individuality of their personalities, and, therefore, the identities of their metaphysical egos, assume the identities of other personalities, and are invested apparently with new ego-natures and mentalities. Thus the ego seems to be a mutable and protean thing, attaching itself to any personage fancy may present with sufficient vividness, or disappearing wholly during moments of intense mental concentration or physical exertion. Perhaps in some phases of insanity and in early childhood, the very sense of being an ego-creature is lacking, and the phantasmagoria of mental states becomes an independent and not an individual experience.

Thus consciousness might overleap the bounds of the ego-self and become a free, cosmic, non-personal and uncircumscribed experience. Some such attitude as this toward the All may be the basis of the Vedantic solipsism involved in the formula: *Brahma-atman-aikyam,* the identity or unity of all selfhoods with the infinite and absolute Brahm. What but something of this nature is the Kantian "Universal Consciousness" (*Bewusstsein überhaupt*)?

But Wheeler's Primordial Mental is consciousless, yet thrilled into unrest by an urge toward self-realization, which culminates, so far as we know, in the mind of man, whose furthest reach toward self-consciousness, even yet, is only a delusive concept, a figment of fancy which is conceived by the real thinker as conscious and even self-conscious.[4] For this thinker is not the object seen, as supposed,

[4] "We do not know with absolute knowledge," says Wheeler, "that there is a thinker behind thinking....but that thinking is its own thinker, that thinking is thinker thinking, or that thought is itself thinker thinking, is quite a

in the critical moment of self-consciousness. That projection of the thinker which results in the concept or idea of a self as contradistinguished from the not-self becomes the thought-object of consciousness, subject and object thus delusively merging or identifying.

Wheeler in his attempt to exploit that presumptive entity, that psychic Achilles in his tent, back of the idea or concept of self, there is employed largely the terminology of Buddhistic psychology. It is not an entelechy or unitary, indiscerptible being, but rather "a group of qualities, having no existence independent of the mind," and dependent for the integrity of its composition upon the containing vital organism. The thinker is perhaps only a set of conditions developed in vital organization, and through which some of the fathomless potentialities inherent in matter manifest themselves in mental phenomena, and of which awareness may be reckoned a response to Reality's urge toward self-realization. What and how the thinker thinks is unquestionably referable to the structural and essential character of brain and nerve tissue.

As to the origin of the ego-concept, Wheeler seems to assign it to nothing more substantial than the recognition of the brain as a haunting presence which, in a nondescript manner, develops in consciousness an indefinite image, entertained conceptually as our ego. The brain is, indeed, the only internal organ impinging with sufficient intimacy upon the functional field of consciousness to impart a direct stimulus. For the existence of all other organs of

little too much for our minds to grasp. It is as inconceivable by us as acting without an actor, or motion without a thing in motion." Assuming there is a thinker distinct from thought, what happens in the supposed experience of self-realization or self-consciousness is this: "There is the thought-subject as well as the thought-object; and the thought-subject thought-conscious and self-conscious. In other words, there is the thinker thinking the subject, and then thinking that thought-subject conscious and self-conscious; exactly as McCready, the actor, lost in the idea of himself as King John, thinks first, in logical order, the King of his imagination, then thinks that creature of his thought conscious and self-conscious, as he must be if beside himself thinking himself that creature."

the body indirect testimony must be adduced; that is, they are purely of objective character, while the brain is, in a sense, semi-subjective. Indeed, Wheeler actually attributes to the cephalic substance a mental consistency, for he says: "The brain must be something mental, as it is absolutely unthinkable that that which should be so correlated with mind should not be itself something mental, even if yet nothing of mind or consciousness."

Thus brain-matter seems to partake of two natures. Its marvelous radio-activity, functioning inward, creates the pageantries of mental states; functioning outward with its neural tentacles, it receives and transmits sensorial material in vibratory symbols from an outlying world. As to the transmission and transmutation of these sensorial data from the substratum of mind into their specific forms amid the content of consciousness, Wheeler has much to say. Indeed this matter forms the crux of his attack upon the metaphysics of Kant.

Wheeler's attitude toward that "scandal of the universe," the problem of evil, presents a suggestion of Fatalism, though high ethical ideals characterize all his thought. His metaphysics of the ethical may, perhaps, be formulated as follows: What exists primordially and permanently exists necessarily, with all its involutions and capacity for evolving. The aboriginal Mental is without consciousness of the type known to human thought. If there exists a universal awareness as related to that which is perceived as the physical, this can have no cognoscence of what may be termed the subjective activities of the cosmos, any more than we cognize the functionings of our own vital organs. Neither could such a pervasive consciousness be, in any wise, responsible for what obtained before it in the unconscious Primordial Mental any more than our consciousness is responsible for the structural conformations and interrelations of our internal organs.

To such a consciousness the relationships and adjustments of the cosmic parts would not become a matter ratiocinated as good or evil; rather would all be good, or a subject of indifference. It is man's reading into the nexus of being the fiction of a universal or infinite personality endowed with qualities existent only as evolved human concepts that gives rise to the problem of evil.[5]

The polemic, *A Critique of Pure Kant,* is an elaborate declaration that, despite the imperfections of the mechanicality of perception, "we never come so near the Absolute Reality, certainly never so primarily and directly near, as we do in sensuous experience. No thinking can bring us so near." The interpretation which he adopts of the Kantian system as it relates to the outer world, is summarized as follows: "What does a man see when he looks, or rather what perceive when he perceives? Is it what primarily he aims at, or something short of it? According to Kant we perceive, indeed, in the direction of an outlying world which he dogmatically assumes and affirms to exist," but with which we fail to connect in any direct contiguity. "We perceive in the direction of such world, but perceive only its effects upon the mind, and nothing of that world itself:—or no, perceive not even those effects upon the mind, but only the mind's effects on those effects; which is to perceive, in fact, not quite those effects on the mind, even."

The doctrine of the phenomenality of our world, and not that of the possibility of knowledge independent of sen-

[5] "Every theist acknowledges the principle, if I may so call it, of something's obtaining of necessity, and claims it for his Being of God, who, if not originating Himself, nor anything originating Him, must obtain of necessity. The only difference between the theist and myself is that he maintains that his Being of God obtains of necessity, and I that it is the universe that does.

"But again, if the universe obtains of necessity, then nothing in it obtains of aboriginal *intent.* And now notice that this accords perfectly with the universe as an evolution, which, as making mind and consciousness a development, debars anything like intent in the beginning. And with no goodness or anything else in intent in the universe, there is left only goodness or such *in effect.*"—From a personal letter.

suous experience, says Wheeler, constitutes the essence of Kant's transcendental Esthetic and Metaphysic. "In Kant's time, as well as in all time before him, the well-nigh universal conviction was a snap creation. . . . Even to-day," says our bellicose author, "the academic hierarchy at the centers of learning, in their deadly Bourbonism, go right on discoursing metaphysics in terms of a snap, or fiat creation." The primacy and transcendency of conscious mind are arbitrarily assumed, and averred to be interrelated with a universal intelligence. *A priori* intuition implies it, and yet the validity of consciousness is questioned or repudiated. The existence of an outside world was regarded by Kant, especially in his latter years, as probable; but between it and consciousness he interposed the buffer of phenomenality.

If doubt exists as to the validity of perception, as an interpreter of the external, why assume that this conscious principle, glancing inward, interprets with any more exactness the subjective phenomena? An intuitional or *a priori* deliverance may be quite as misleading, albeit quite as sincere, as is the eye's message that the sun moves in the heavens, and that the grass is green at our feet. It has been Wheeler's chief endeavor to discover and dissolve the fundamental illusions of the understanding, which are old as apperception, and as persistent as is the delusion of the sun's motion. Thus he attacks our concept of the ego, our illusion as to self-consciousness, and other errors of intuition and understanding.

No philosophy, says Wheeler, can stand permanently upon any less a foundation than that of the three primary facts of consciousness: recognition of an outlying world absolute, sensuously perceptible; awareness of our own existence; and the additional recognition of the existence of other minds than our own. Yet never in the history

of speculation has a philosopher based his system upon all these primary facts.

Physical demonstrations of the existence of an outside world, and our veritable perception thereof, are essayed by Wheeler. Time and space are likewise regarded as entities, and not forms of the sensibility or conception. "These innate *a priori* forms, conceptions or what not are no more necessary conditions of cognition, no more a subjective necessity of it, than they are of a glass mirror that it should cognosce objects in order to reflect them.... Not finding time and space 'given' in the sensations, Kant sets himself the task of discovering them elsewhere.... He does not, as he expected, find the eggs in the hennery, and so is warranted, he thinks, in assuming them laid in a tree-top by a crow." Not finding them in the sensations, he arbitrarily, and without valid logical process, relegates them to the sensibility.

Of sensibility Wheeler says it is a faculty, not an activity; but "an intuition, *a priori* or any other is an activity, a form of perceiving. And so when Kant says, space, for example, is a form of the sensibility, and in the same breath that it is an intuition, he contradicts himself." Sensibility is but the potentiality of which sensation is the kinetic exponent. If the concepts space and time lay in the sensibility only "as a symphony in an orchestra, how should we have advance notice of their being there? and, indeed, what need of an *a priori* divining of space and time in the sensibility in advance of their obtaining in actuality in the sensations?"

There is implicit in Kant's doctrine of the *a priori* the assumption that as mind is it always has been since its "creation"—a complete, unitary faculty—an entelechy—a thing of primordial perfection—a Minerva sprung from the head of Jove. Let there be Mind! And there was Mind—so complete, so furnished that it can forestall the

functional yieldings of all developing faculty, all sensuous deliverances, all products of the toilsomely logical activities of the understanding. But the fruit does not determine the character of the flower and the leaf. "Why then," asks Wheeler, "should we expect the faculty for entertaining things *as such,* or as abstractions, the faculty, namely, of the understanding, before it exists to meddlesomely interfere and determine the nature and functioning of the faculty for sensations, the faculty, namely, of the sensibility?" For here the concept is assumed to exist before the sensuous experience that creates it.

One of Kant's interpreters is quoted by Wheeler as saying, "Space is not an object of sense, but a fundamental *con*ception that makes external *per*ception possible." Then, says Wheeler, "the flea and the fly have no external perception unless they are endowed with one of the highest faculties of the mind, the power to entertain concepts. But they have external perception, and have it in space and time; not as things conceived, but as things primarily realized and perceived, whether subsequently conceived or not."

This in part constitutes Wheeler's account of the absoluteness of time and space, and the identification of these with the conditions logically and physically demanded for the existence of that external world which Kant, in common with all rational minds, believed to exist.

In like manner Wheeler has applied his catalytic Mind-not-aboriginal in efforts to disintegrate and destroy the logical integrity of all idealistic theories.

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THE PHILOSOPHY OF LOGICAL ATOMISM.

(The following articles are the first two lectures of a course of eight lectures delivered in London in the first months of 1918, and are very largely concerned with explaining certain ideas which I learnt from my friend and former pupil Ludwig Wittgenstein. I have had no opportunity of knowing his views since August, 1914, and I do not even know whether he is alive or dead. He has therefore no responsibility for what is said in these lectures beyond that of having originally supplied many of the theories contained in them. The six other lectures will appear in the three following numbers of *The Monist*.—B. R.)

I. FACTS AND PROPOSITIONS.

THIS course of lectures which I am now beginning I have called the Philosophy of Logical Atomism. Perhaps I had better begin by saying a word or two as to what I understand by that title. The kind of philosophy that I wish to advocate, which I call Logical Atomism, is one which has forced itself upon me in the course of thinking about the philosophy of mathematics, although I should find it hard to say exactly how far there is a definite logical connection between the two. The things I am going to say in these lectures are mainly my own personal opinions and I do not claim that they are more than that.

As I have attempted to prove in *The Principles of Mathematics,* when we analyze mathematics we bring it all back to logic. It all comes back to logic in the strictest and most formal sense. In the present lectures, I shall try to set forth in a sort of outline, rather briefly and rather

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unsatisfactorily, a kind of logical doctrine which seems to me to result from the philosophy of mathematics—not exactly logically, but as what emerges as one reflects: a certain kind of logical doctrine, and on the basis of this a certain kind of metaphysic. The logic which I shall advocate is atomistic, as opposed to the monistic logic of the people who more or less follow Hegel. When I say that my logic is atomistic, I mean that I share the common-sense belief that there are many separate things; I do not regard the apparent multiplicity of the world as consisting merely in phases and unreal divisions of a single indivisible Reality. It results from that that a considerable part of what one would have to do to justify the sort of philosophy I wish to advocate would consist in justifying the process of analysis. One is often told that the process of analysis is falsification, that when you analyze any given concrete whole you falsify it and that the results of analysis are not true. I do not think that is a right view. I do not mean to say, of course, and nobody would maintain, that when you have analyzed you keep everything that you had before you analyzed. If you did, you would never attain anything in analyzing. I do not propose to meet the views that I disagree with by controversy, by arguing against those views, but rather by positively setting forth what I believe to be the truth about the matter, and endeavoring all the way through to make the views that I advocate result inevitably from absolutely undeniable data. When I talk of "undeniable data" that is not to be regarded as synonymous with "true data," because "undeniable" is a psychological term and "true" is not. When I say that something is "undeniable," I mean that it is not the sort of thing that anybody is going to deny; it does not follow from that that it is true, though it does follow that we shall all think it true—and that is as near to truth as we seem able to get. When you are considering any sort of theory of knowledge,

you are more or less tied to a certain unavoidable subjectivity, because you are not concerned simply with the question what is true of the world, but "What can I know of the world?" You always have to start any kind of argument from something which appears to you to be true; if it appears to you to be true, there is no more to be done. You cannot go outside yourself and consider abstractly whether the things that appear to you to be true are true; you may do this in a particular case, where one of your beliefs is changed in consequence of others among your beliefs.

The reason that I call my doctrine *logical* atomism is because the atoms that I wish to arrive at as the sort of last residue in analysis are logical atoms and not physical atoms. Some of them will be what I call "particulars,"—such things as little patches of color or sounds, momentary things—and some of them will be predicates or relations and so on. The point is that the atom I wish to arrive at is the atom of logical analysis, not the atom of physical analysis.

It is a rather curious fact in philosophy that the data which are undeniable to start with are always rather vague and ambiguous. You can, for instance, say: "There are a number of people in this room at this moment." That is obviously in some sense undeniable. But when you come to try and define what this room is, and what it is for a person to be in a room, and how you are going to distinguish one person from another, and so forth, you find that what you have said is most fearfully vague and that you really do not know what you meant. That is a rather singular fact, that everything you are really sure of, right off is something that you do not know the meaning of, and the moment you get a precise statement you will not be sure whether it is true or false, at least right off. The process of sound philosophizing, to my mind, consists mainly in passing from those obvious, vague, ambiguous things,

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that we feel quite sure of, to something precise, clear, definite, which by reflection and analysis we find is involved in the vague thing that we started from, and is, so to speak, the real truth of which that vague thing is a sort of shadow. I should like, if time were longer and if I knew more than I do, to spend a whole lecture on the conception of vagueness. I think vagueness is very much more important in the theory of knowledge than you would judge it to be from the writings of most people. Everything is vague to a degree you do not realize till you have tried to make it precise, and everything precise is so remote from everything that we normally think, that you cannot for a moment suppose that is what we really mean when we say what we think.

When you pass from the vague to the precise by the method of analysis and reflection that I am speaking of, you always run a certain risk of error. If I start with the statement that there are so and so many people in this room, and then set to work to make that statement precise, I shall run a great many risks and it will be extremely likely that any precise statement I make will be something not true at all. So you cannot very easily or simply get from these vague undeniable things to precise things which are going to retain the undeniability of the starting-point. The precise propositions that you arrive at may be *logically* premises to the system that you build up upon the basis of them, but they are not premises for the theory of knowledge. It is important to realize the difference between that from which your knowledge is, in fact, derived, and that from which, if you already had complete knowledge, you would deduce it. Those are quite different things. The sort of premise that a logician will take for a science will not be the sort of thing which is first known or easiest known: it will be a proposition having great deductive power, great cogency and exactitude, quite a different thing from the

actual premise that your knowledge started from. When you are talking of the premise for theory of knowledge, you are not talking of anything objective, but of something that will vary from man to man, because the premises of one man's theory of knowledge will not be the same as those of another man's. There is a great tendency among a very large school to suppose that when you are trying to philosophize about what you know, you ought to carry back your premises further and further into the region of the inexact and vague, beyond the point where you yourself are, right back to the child or monkey, and that anything whatsoever that *you* seem to know—but that the psychologist recognizes as being the product of previous thought and analysis and reflection on your part—cannot really be taken as a premise in your own knowledge. That, I say, is a theory which is very widely held and which is used against that kind of analytic outlook which I wish to urge. It seems to me that when your object is, not simply to study the history or development of mind, but to ascertain the nature of the world, you do not want to go any further back than you are already yourself. You do not want to go back to the vagueness of the child or monkey, because you will find that quite sufficient difficulty is raised by your own vagueness. But there one is confronted by one of those difficulties that occur constantly in philosophy, where you have two ultimate prejudices conflicting and where argument ceases. There is the type of mind which considers that what is called primitive experience must be a better guide to wisdom than the experience of reflective persons, and there is the type of mind which takes exactly the opposite view. On that point I cannot see any argument whatsoever. It is quite clear that a highly educated person sees, hears, feels, does everything in a very different way from a young child or animal, and that this whole manner of experiencing the world and of thinking about the world is

very much more analytic than that of a more primitive experience. The things we have got to take as premises in any kind of work of analysis are the things which appear to *us* undeniable—to us here and now, as we are—and I think on the whole that the sort of method adopted by Descartes is right: that you should set to work to doubt things and retain only what you cannot doubt because of its clearness and distinctness, not because you are sure not to be induced into error, for there does not exist a method which will safeguard you against the possibility of error. The wish for perfect security is one of those snares we are always falling into, and is just as untenable in the realm of knowledge as in everything else. Nevertheless, granting all this, I still think that Descartes's method is on the whole a sound one for the starting-point.

I propose, therefore, always to begin any argument that I have to make by appealing to data which will be quite ludicrously obvious. Any philosophical skill that is required will consist in the selection of those which are capable of yielding a good deal of reflection and analysis, and in the reflection and analysis themselves.

What I have said so far is by way of introduction.

The first truism to which I wish to draw your attention —and I hope you will agree with me that these things that I call truisms are so obvious that it is almost laughable to mention them—is that the world contains facts, which are what they are whatever we may choose to think about them, and that there are also beliefs, which have reference to facts, and by reference to facts are either true or false. I will try first of all to give you a preliminary explanation of what I mean by a "fact." When I speak of a fact—I do not propose to attempt an exact definition, but an explanation, so that you will know what I am talking about—I mean the kind of thing that makes a proposition true or false. If I say "It is raining," what I say is true

in a certain condition of weather and is false in other conditions of weather. The condition of weather that makes my statement true (or false as the case may be), is what I should call a "fact." If I say "Socrates is dead," my statement will be true owing to a certain physiological occurrence which happened in Athens long ago. If I say, "Gravitation varies inversely as the square of the distance," my statement is rendered true by astronomical fact. If I say, "Two and two are four," it is arithmetical fact that makes my statement true. On the other hand, if I say "Socrates is alive," or "Gravitation varies directly as the distance," or "Two and two are five," the very same facts which made my previous statements true show that these new statements are false.

I want you to realize that when I speak of a fact I do not mean a particular existing thing, such as Socrates or the rain or the sun. Socrates himself does not render any statement true or false. You might be inclined to suppose that all by himself he would give truth to the statement "Socrates existed," but as a matter of fact that is a mistake. It is due to a confusion which I shall try to explain in the sixth lecture of this course, when I come to deal with the notion of existence. Socrates[1] himself, or any particular thing just by itself, does not make any proposition true or false. "Socrates is dead" and "Socrates is alive" are both of them statements about Socrates. One is true and the other false. What I call a fact is the sort of thing that is expressed by a whole sentence, not by a single name like "Socrates." When a single word does come to express a fact, like "fire" or "wolf," it is always due to an unexpressed context, and the full expression of a fact will always involve a sentence. We express a fact, for example, when we say that a certain thing has a certain property,

[1] I am here for the moment treating Socrates as a "particular." But we shall see shortly that this view requires modification.

or that it has a certain relation to another thing; but the thing which has the property or the relation is not what I call a "fact."

It is important to observe that facts belong to the objective world. They are not created by our thoughts or beliefs except in special cases. That is one of the sort of things which I should set up as an obvious truism, but, of course, one is aware, the moment one has read any philosophy at all, how very much there is to be said before such a statement as that can become the kind of position that you want. The first thing I want to emphasize is that the outer world—the world, so to speak, which knowledge is aiming at knowing—is not completely described by a lot of "particulars," but that you must also take account of these things that I call facts, which are the sort of things that you express by a sentence, and that these, just as much as particular chairs and tables, are part of the real world. Except in psychology, most of our statements are not intended merely to express our condition of mind, though that is often all that they succeed in doing. They are intended to express facts, which (except when they are psychological facts) will be about the outer world. There are such facts involved, equally when we speak truly and when we speak falsely. When we speak falsely it is an objective fact that makes what we say true when we speak truly.

There are a great many different kinds of facts, and we shall be concerned in later lectures with a certain amount of classification of facts. I will just point out a few kinds of facts to begin with, so that you may not imagine that facts are all very much alike. There are particular facts, such as "This is white"; then there are general facts, such as "All men are mortal." Of course, the distinction between particular and general facts is one of the most important. There again it would be a very great mistake to suppose that you could describe the world completely by means of

particular facts alone. Suppose that you had succeeded in chronicling every single particular fact throughout the universe, and that there did not exist a single particular fact of any sort anywhere that you had not chronicled, you still would not have got a complete description of the universe unless you also added: "These that I have chronicled are all the particular facts there are." So you cannot hope to describe the world completely without having general facts as well as particular facts. Another distinction, which is perhaps a little more difficult to make, is between positive facts and negative facts, such as "Socrates was alive"—a positive fact,— and "Socrates is not alive"—you might say a negative fact.[2] But the distinction is difficult to make precise. Then there are facts concerning particular things or particular qualities or relations, and, apart from them, the completely general facts of the sort that you have in logic, where there is no mention of any constituent whatever of the actual world, no mention of any particular thing or particular quality or particular relation, indeed strictly you may say no mention of anything. That is one of the characteristics of logical propositions, that they mention nothing. Such a proposition is: "If one class is part of another, a term which is a member of the one is also a member of the other." All those words that come in the statement of a pure logical proposition are words really belonging to syntax. They are words merely expressing form or connection, not mentioning any particular constituent of the proposition in which they occur. This is, of course, a thing that wants to be proved; I am not laying it down as self-evident. Then there are facts about the properties of single things; and facts about the relations between two things, three things, and so on; and any number of different classifications of some of the facts in the world, which are important for different purposes.

[2] Negative facts are further discussed in a later lecture.

It is obvious that there is not a dualism of true and false facts; there are only just facts. It would be a mistake, of course, to say that all facts are true. That would be a mistake because true and false are correlatives, and you would only say of a thing that it was true if it was the sort of thing that *might* be false. A fact cannot be either true or false. That brings us on to the question of statements or propositions or judgments, all those things that do have the duality of truth and falsehood. For the purposes of logic, though not, I think, for the purposes of theory of knowledge, it is natural to concentrate upon the proposition as the thing which is going to be our typical vehicle on the duality of truth and falsehood. A proposition, one may say, is a sentence in the indicative, a sentence asserting something, not questioning or commanding or wishing. It may also be a sentence of that sort preceded by the word "that." For example, "That Socrates is alive," "That two and two are four," "That two and two are five," anything of that sort will be a proposition.

A proposition is just a symbol. It is a complex symbol in the sense that it has parts which are also symbols: a symbol may be defined as complex when it has parts that are symbols. In a sentence containing several words, the several words are each symbols, and the sentence composing them is therefore a complex symbol in that sense. There is a good deal of importance to philosophy in the theory of symbolism, a good deal more than at one time I thought. I think the importance is almost entirely negative, i. e., the importance lies in the fact that unless you are fairly self-conscious about symbols, unless you are fairly aware of the relation of the symbol to what it symbolizes, you will find yourself attributing to the thing properties which only belong to the symbol. That, of course, is especially likely in very abstract studies such as philosophical logic, because the subject-matter that you are supposed to

be thinking of is so exceedingly difficult and elusive that any person who has ever tried to think about it knows you do not think about it except perhaps once in six months for half a minute. The rest of the time you think about the symbols, because they are tangible, but the thing you are supposed to be thinking about is fearfully difficult and one does not often manage to think about it. The really good philosopher is the one who does once in six months think about it for a minute. Bad philosophers never do. That is why the theory of symbolism has a certain importance, because otherwise you are so certain to mistake the properties of the symbolism for the properties of the thing. It has other interesting sides to it too. There are different kinds of symbols, different kinds of relation between symbol and what is symbolized, and very important fallacies arise from not realizing this. The sort of contradictions about which I shall be speaking in connection with types in a later lecture all arise from mistakes in symbolism, from putting one sort of symbol in the place where another sort of symbol ought to be. Some of the notions that have been thought absolutely fundamental in philosophy have arisen, I believe, entirely through mistakes as to symbolism—e. g., the notion of existence, or, if you like, reality. Those two words stand for a great deal that has been discussed in philosophy. There has been the theory about every proposition being really a description of reality as a whole and so on, and altogether these notions of reality and existence have played a very prominent part in philosophy. Now my own belief is that as they have occurred in philosophy, they have been entirely the outcome of a muddle about symbolism, and that when you have cleared up that muddle, you find that practically everything that has been said about existence is sheer and simple mistake, and that is all you can say about it. I shall go into that in

a later lecture, but it is an example of the way in which symbolism is important.

Perhaps I ought to say a word or two about what I am understanding by symbolism, because I think some people think you only mean mathematical symbols when you talk about symbolism. I am using it in a sense to include all language of every sort and kind, so that every word is a symbol, and every sentence, and so forth. When I speak of a symbol I simply mean something that "means" something else, and as to what I mean by "meaning" I am not prepared to tell you. I will in the course of time enumerate a strictly infinite number of different things that "meaning" may mean, but I shall not consider that I have exhausted the discussion by doing that. I think that the notion of meaning is always more or less psychological, and that it is not possible to get a pure logical theory of meaning, nor therefore of symbolism. I think that it is of the very essence of the explanation of what you mean by a symbol to take account of such things as knowing, of cognitive relations, and probably also of association. At any rate I am pretty clear that the theory of symbolism and the use of symbolism is not a thing that can be explained in pure logic without taking account of the various cognitive relations that you may have to things.

As to what one means by "meaning," I will give a few illustrations. For instance, the word "Socrates," you will say, means a certain man; the word "mortal" means a certain quality; and the sentence "Socrates is mortal" means a certain fact. But these three sorts of meaning are entirely distinct, and you will get into the most hopeless contradictions if you think the word "meaning" has the same meaning in each of these three cases. It is very important not to suppose that there is just one thing which is meant by "meaning," and that therefore there is just one sort of relation of the symbol to what is symbolized.

A name would be a proper symbol to use for a person; a sentence (or a proposition) is the proper symbol for a fact.

A belief or a statement has duality of truth and falsehood, which the fact does not have. A belief or a statement always involves a proposition. You say that a man believes that so and so is the case. A man believes that Socrates is dead. What he believes is a proposition on the face of it, and for formal purposes it is convenient to take the proposition as the essential thing having the duality of truth and falsehood. It is very important to realize such things, for instance, as that propositions are not names for facts. It is quite obvious as soon as it is pointed out to you, but as a matter of fact I never had realized it until it was pointed out to me by a former pupil of mine, Wittgenstein. It is perfectly evident as soon as you think of it, that a proposition is not a name for a fact, from the mere circumstance that there are *two* propositions corresponding to each fact. Suppose it is a fact that Socrates is dead. You have two propositions: "Socrates is dead" and "Socrates is not dead." And those two propositions corresponding to the same fact, there is one fact in the world which makes one true and one false. That is not accidental, and illustrates how the relation of proposition to fact is a totally different one from the relation of name to the thing named. For each fact there are two propositions, one true and one false, and there is nothing in the nature of the symbol to show us which is the true one and which is the false one. If there were, you could ascertain the truth about the world by examining propositions without looking round you.

There are two different relations, as you see, that a proposition may have to a fact: the one the relation that you may call being true to the fact, and the other being false to the fact. Both are equally essentially logical relations which may subsist between the two, whereas in the case of a name, there is only one relation that it can have

to what it names. A name can just name a particular, or, if it does not, it is not a name at all, it is a noise. It cannot be a name without having just that one particular relation of naming a certain thing, whereas a proposition does not cease to be a proposition if it is false. It has these two ways, of being true and being false, which together correspond to the property of being a name. Just as a word may be a name or be not a name but just a meaningless noise, so a phrase which is apparently a proposition may be either true or false, or may be meaningless, but the true and false belong together as against the meaningless. That shows, of course, that the formal logical characteristics of propositions are quite different from those of names, and that the relations they have to facts are quite different, and therefore propositions are not names for facts. You must not run away with the idea that you can name facts in any other way; you cannot. You cannot name them at all. You cannot properly name a fact. The only thing you can do is to assert it, or deny it, or desire it, or will it, or wish it, or question it, but all those are things involving the whole proposition. You can never put the sort of thing that makes a proposition to be true or false in the position of a logical subject. You can only have it there as something to be asserted or denied or something of that sort, but not something to be named.

DISCUSSION.

..... Do you take your starting-point "That there are many things" as a postulate which is to be carried along all through, or has to be proved afterward?

Mr. Russell: No, neither the one nor the other. I do not take it as a postulate that "There are many things." I should take it that, in so far as it can be proved, the proof is empirical, and that the disproofs that have been offered are *a priori*. The empirical person would naturally say, there are many things. The monistic philosopher attempts to show that there are not. I should propose to refute his *a priori* arguments.

I do not consider there is any *logical* necessity for there to be many things, nor for there not to be many things.

..... I mean in making a start, whether you start with the empirical or the *a priori* philosophy, do you make your statement just at the beginning and come back to prove it, or do you never come back to the proof of it?

Mr. Russell: No, you never come back. It is like the acorn to the oak. You never get back to the acorn in the oak. I should like a statement which would be rough and vague and have that sort of obviousness that belongs to things of which you never know what they mean, but I should never get back to that statement. I should say, here is a thing. We seem somehow convinced that there is truth buried in this thing somewhere. We will look at it inside and out until we have extracted something and can say, now that is true. It will not really be the same as the thing we started from because it will be so much more analytic and precise.

..... Does it not look as though you could name a fact by a date?

Mr. Russell: You can apparently name facts, but I do not think you can really: you would always find that if you set out the whole thing fully, it was not so. Suppose you say "The death of Socrates." You might say, that is a name for the fact that Socrates died. But it obviously is not. You can see that the moment you take account of truth and falsehood. Supposing he had not died, the phrase would still be just as significant although there could not be then anything you could name. But supposing he had never lived, the sound "Socrates" would not be a name at all. You can see it in another way. You can say "The death of Socrates is a fiction." Suppose you had read in the paper that the Kaiser had been assassinated, and it turned out to be not true. You could then say, "The death of the Kaiser is a fiction." It is clear that there is no such thing in the world as a fiction, and yet that statement is a perfectly sound statement. From this it follows that "The death of the Kaiser" is not a name.

II. PARTICULARS, PREDICATES, AND RELATIONS.

I propose to begin to-day the analysis of facts and propositions, for in a way the chief thesis that I have to main-

tain is the legitimacy of analysis, because if one goes into what I call Logical Atomism that means that one does believe the world can be analyzed into a number of separate things with relations and so forth, and that the sort of arguments that many philosophers use against analysis are not justifiable.

In a philosophy of logical atomism one might suppose that the first thing to do would be to discover the kinds of atoms out of which logical structures are composed. But I do not think that is quite the first thing; it is one of the early things, but not quite the first. There are two other questions that one has to consider, and one of these at least is prior. You have to consider:

1. Are the things that look like logically complex entities really complex?
2. Are they really entities?

The second question we can put off; in fact, I shall not deal with it fully until my last lecture. The first question, whether they are really complex, is one that you have to consider at the start. Neither of these questions is, as it stands, a very precise question. I do not pretend to start with precise questions. I do not think you can start with anything precise. You have to achieve such precision as you can, as you go along. Each of these two questions, however, is *capable* of a precise meaning, and each is really important.

There is another question which comes still earlier, namely: what shall we take as *prima facie* examples of logically complex entities? That really is the first question of all to start with. What sort of things shall we regard as *prima facie* complex?

Of course, all the ordinary objects of daily life are apparently complex entities: such things as tables and chairs,

loaves and fishes, persons and principalities and powers—they are all on the face of it complex entities. All the kinds of things to which we habitually give proper names are on the face of them complex entities: Socrates, Piccadilly, Rumania, Twelfth Night or anything you like to think of, to which you give a proper name, they are all apparently complex entities. They seem to be complex systems bound together into some kind of a unity, that sort of a unity that leads to the bestowal of a single appellation. I think it is the contemplation of this sort of apparent unity which has very largely led to the philosophy of monism, and to the suggestion that the universe as a whole is a single complex entity more or less in the sense in which these things are that I have been talking about.

For my part, I do not believe in complex entities of this kind, and it is not such things as these that I am going to take as the *prima facie* examples of complex entities. My reasons will appear more and more plainly as I go on. I cannot give them all to-day, but I can more or less explain what I mean in a preliminary way. Suppose, for example, that you were to analyze what appears to be a fact about Piccadilly. Suppose you made any statement about Piccadilly, such as: "Piccadilly is a pleasant street." If you analyze a statement of that sort correctly, I believe you will find that the fact corresponding to your statement does not contain any constituent corresponding to the word "Piccadilly." The word "Piccadilly" will form part of many significant propositions, but the facts corresponding to these propositions do not contain any single constituent, whether simple or complex, corresponding to the word "Piccadilly." That is to say, if you take language as a guide in your analysis of the fact expressed, you will be led astray in a statement of that sort. The reasons for that I shall give at length in Lecture VI, and partly also in Lecture VII, but I could say in a preliminary way certain things that would

make you understand what I mean. "Piccadilly," on the face of it, is the name for a certain portion of the earth's surface, and I suppose, if you wanted to define it, you would have to define it as a series of classes of material entities, namely those which, at varying times, occupy that portion of the earth's surface. So that you would find that the logical status of Piccadilly is bound up with the logical status of series and classes, and if you are going to hold Piccadilly as real, you must hold that series of classes are real, and whatever sort of metaphysical status you assign to them, you must assign to it. As you know, I believe that series and classes are of the nature of logical fictions: therefore that thesis, if it can be maintained, will dissolve Piccadilly into a fiction. Exactly similar remarks will apply to other instances: Rumania, Twelfth Night, and Socrates. Socrates, perhaps, raises some special questions, because the question what constitutes a person has special difficulties in it. But, for the sake of argument, one might identify Socrates with the series of his experiences. He would be really a series of classes, because one has many experiences simultaneously. Therefore he comes to be very like Piccadilly.

Considerations of that sort seem to take us away from such *prima facie* complex entities as we started with to others as being more stubborn and more deserving of analytic attention, namely facts. I explained last time what I meant by a fact, namely, that sort of thing that makes a proposition true or false, the sort of thing which is the case when your statement is true and is not the case when your statement is false. Facts are, as I said last time, plainly something you have to take account of if you are going to give a complete account of the world. You cannot do that by merely enumerating the particular things that are in it: you must also mention the relations of these things, and their properties, and so forth, all of which are

facts, so that facts certainly belong to an account of the objective world, and facts do seem much more clearly complex and much more not capable of being explained away than things like Socrates and Rumania. However you may explain away the meaning of the word "Socrates," you will still be left with the truth that the proposition "Socrates is mortal" expresses a fact. You may not know exactly what Socrates means, but it is quite clear that "Socrates is mortal" does express a fact. There is clearly some valid meaning in saying that the fact expressed by "Socrates is mortal" is *complex*. The things in the world have various properties, and stand in various relations to each other. That they have these properties and relations are *facts,* and the things and their qualities or relations are quite clearly in some sense or other components of the facts that have those qualities or relations. The analysis of apparently complex *things* such as we started with can be reduced by various means, to the analysis of facts which are apparently about those things. Therefore it is with the analysis of *facts* that one's consideration of the problem of complexity must begin, not by the analysis of apparently complex things.

The complexity of a fact is evidenced, to begin with, by the circumstance that the proposition which asserts a fact consists of several words, each of which may occur in other contexts. Of course, sometimes you get a proposition expressed by a single word, but if it is expressed fully it is bound to contain several words. The proposition "Socrates is mortal" may be replaced by "Plato is mortal" or by "Socrates is human"; in the first case we alter the subject, in the second the predicate. It is clear that all the propositions in which the word "Socrates" occurs have something in common, and again all the propositions in which the word "mortal" occurs have something in common, something which they do not have in common with

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all facts but only to those which are about Socrates or mortality. It is clear, I think, that the facts corresponding to propositions in which the word "Socrates" occurs have something in common corresponding to the common word "Socrates" which occurs in the propositions, so that you have that sense of complexity to begin with, that in a fact you can get something which it may have in common with other facts, just as you may have "Socrates is human" and "Socrates is mortal," both of them facts, and both having to do with Socrates, although Socrates does not constitute the whole of either of these facts. It is quite clear that in that sense there is a possibility of cutting up a fact into component parts, of which one component may be altered without altering the others, and one component may occur in certain other facts though not in all other facts. I want to make it clear, to begin with, that there is a sense in which facts can be analyzed. I am not concerned with all the difficulties of any analysis, but only with meeting the *prima facie* objections of philosophers who think you really cannot analyze at all.

I am trying as far as possible again this time, as I did last time, to start with perfectly plain truisms. My desire and wish is that the things I start with should be so obvious that you wonder why I spend my time stating them. That is what I aim at, because the point of philosophy is to start with something so simple as not to seem worth stating, and to end with something so paradoxical that no one will believe it.

One *prima facie* mark of complexity in propositions is the fact that they are expressed by several words. I come now to another point, which applies primarily to propositions and thence derivatively to facts. You can understand a proposition when you understand the words of which it is composed even though you never heard the proposition before. That seems a very humble property, but it is a

property which marks it as complex and distinguishes it from words whose meaning is simple. When you know the vocabulary, grammar, and syntax of a language, you can understand a proposition in that language even though you never saw it before. In reading a newspaper, for example, you become aware of a number of statements which are new to you, and they are intelligible to you immediately, in spite of the fact that they are new, because you understand the words of which they are composed. This characteristic, that you can understand a proposition through the understanding of its component words, is absent from the component words when those words express something simple. Take the word "red," for example, and suppose —as one always has to do—that "red" stands for a particular shade of color. You will pardon that assumption, but one never can get on otherwise. You cannot understand the meaning of the word "red" except through seeing red things. There is no other way in which it can be done. It is no use to learn languages, or to look up dictionaries. None of these things will help you to understand the meaning of the word "red." In that way it is quite different from the meaning of a proposition. Of course, you can give a definition of the word "red," and here it is very important to distinguish between a definition and an analysis. All analysis is only possible in regard to what is complex, and it always depends, in the last analysis, upon direct acquaintance with the objects which are the meanings of certain simple symbols. It is hardly necessary to observe that one does not define a thing but a symbol. (A "simple" symbol is a symbol whose parts are not symbols.) A simple symbol is quite a different thing from a simple thing. Those objects which it is impossible to symbolize otherwise than by simple symbols may be called "simple," while those which can be symbolized by a combination of symbols may be called "complex." This is, of course, a preliminary

definition, and perhaps somewhat circular, but that does not much matter at this stage.

I have said that "red" could not be understood except by seeing red things. You might object to that on the ground that you can define red, for example, as "The color with the greatest wave-length." That, you might say, is a definition of "red" and a person could understand that definition even if he had seen nothing red, provided he understood the physical theory of color. But that does not really constitute the meaning of the word "red" in the very slightest. If you take such a proposition as "This is red" and substitute for it "This has the color with the greatest wave-length," you have a different proposition altogether. You can see that at once, because a person who knows nothing of the physical theory of color can understand the proposition "This is red," and can know that it is true, but cannot know that "This has the color which has the greatest wave-length." Conversely, you might have a hypothetical person who could not see red, but who understood the physical theory of color and could apprehend the proposition "This has the color with the greatest wave-length," but who would not be able to understand the proposition "This is red," as understood by the normal uneducated person. Therefore it is clear that if you define "red" as "The color with the greatest wave-length" you are not giving the actual meaning of the word at all; you are simply giving a true description, which is quite a different thing, and the propositions which result are different propositions from those in which the word "red" occurs. In that sense the word "red" cannot be defined, though in the sense in which a correct description constitutes a definition it can be defined. In the sense of analysis you cannot define "red." That is how it is that dictionaries are able to get on, because a dictionary professes to define all the words in the language by means of words in the language, and therefore

it is clear that a dictionary must be guilty of a vicious circle somewhere, but it manages it by means of correct descriptions.

I have made it clear, then, in what sense I should say that the word "red" is a simple symbol and the phrase "This is red" a complex symbol. The word "red" can only be understood through acquaintance with the object, whereas the phrase "Roses are red" can be understood if you know what "red" is and what "roses" are, without ever having heard the phrase before. That is a clear mark of what is complex. It is the mark of a complex symbol, and also the mark of the object symbolized by the complex symbol. That is to say, propositions are complex symbols, and the facts they stand for are complex.

The whole question of the meaning of words is very full of complexities and ambiguities in ordinary language. When one person uses a word, he does not mean by it the same thing as another person means by it. I have often heard it said that that is a misfortune. That is a mistake. It would be absolutely fatal if people meant the same things by their words. It would make all intercourse impossible, and language the most hopeless and useless thing imaginable, because the meaning you attach to your words must depend on the nature of the objects you are acquainted with, and since different people are acquainted with different objects, they would not be able to talk to each other unless they attached quite different meanings to their words. We should have to talk only about logic—a not wholly undesirable result. Take, for example, the word "Piccadilly." We, who are acquainted with Piccadilly, attach quite a different meaning to that word from any which could be attached to it by a person who had never been in London: and, supposing that you travel in foreign parts and expatiate on Piccadilly, you will convey to your hearers entirely different propositions from those in your

mind. They will know Piccadilly as an important street in London; they may know a lot about it, but they will not know just the things one knows when one is walking along it. If you were to insist on language which was unambiguous, you would be unable to tell people at home what you had seen in foreign parts. It would be altogether incredibly inconvenient to have an unambiguous language, and therefore mercifully we have not got one.

Analysis is not the same thing as definition. You can define a term by means of a correct description, but that does not constitute an analysis. It is analysis, not definition, that we are concerned with at the present moment, so I will come back to the question of analysis.

We may lay down the following provisional definitions:

> That the components of a proposition are the symbols we must understand in order to understand the proposition;
>
> That the components of the fact which makes a proposition true or false, as the case may be, are the *meanings* of the symbols which we must understand in order to understand the proposition.

That is not absolutely correct, but it will enable you to understand my meaning. One reason why it fails of correctness is that it does not apply to words which, like "or" and "not," are parts of propositions without corresponding to any part of the corresponding facts. This is a topic for Lecture III.

I call these definitions *preliminary* because they start from the complexity of the proposition, which they define psychologically, and proceed to the complexity of the fact, whereas it is quite clear that in an orderly, proper procedure it is the complexity of the fact that you would start from. It is also clear that the complexity of the fact cannot be something merely psychological. If in astronomical

fact the earth moves round the sun, that is genuinely complex. It is not that you think it complex, it is a sort of genuine objective complexity, and therefore one ought in a proper, orderly procedure to start from the complexity of the world and arrive at the complexity of the proposition. The only reason for going the other way round is that in all abstract matters symbols are easier to grasp. I doubt, however, whether complexity, in that fundamental objective sense in which one starts from complexity of a fact, is definable at all. You cannot analyze what you mean by complexity in that sense. You must just apprehend it—at least so I am inclined to think. There is nothing one could say about it, beyond giving criteria such as I have been giving. Therefore, when you cannot get a real proper analysis of a thing, it is generally best to talk round it without professing that you have given an exact definition.

It might be suggested that complexity is essentially to do with symbols, or that it is essentially psychological. I do not think it would be possible seriously to maintain either of these views, but they are the sort of views that will occur to one, the sort of thing that one would try, to see whether it would work. I do not think they will do at all. When we come to the principles of symbolism which I shall deal with in Lecture VII, I shall try to persuade you that in a logically correct symbolism there will always be a certain fundamental identity of structure between a fact and the symbol for it; and that the complexity of the symbol corresponds very closely with the complexity of the facts symbolized by it. Also, as I said before, it is quite directly evident to inspection that the fact, for example, that two things stand in a certain relation to one another—e. g., that this is to the left of that—is itself objectively complex, and not merely that the apprehension of it is complex. The fact that two things stand in a certain relation to each other, or any statement of that sort, has a

complexity all of its own. I shall therefore in future assume that there is an objective complexity in the world, and that it is mirrored by the complexity of propositions.

A moment ago I was speaking about the great advantages that we derive from the logical imperfections of language, from the fact that our words are all ambiguous. I propose now to consider what sort of language a logically perfect language would be. In a logically perfect language the words in a proposition would correspond one by one with the components of the corresponding fact, with the exception of such words as "or," "not," "if," "then," which have a different function. In a logically perfect language, there will be one word and no more for every simple object, and everything that is not simple will be expressed by a combination of words, by a combination derived, of course, from the words for the simple things that enter in, one word for each simple component. A language of that sort will be completely analytic, and will show at a glance the logical structure of the facts asserted or denied. The language which is set forth in *Principia Mathematica* is intended to be a language of that sort. It is a language which has only syntax and no vocabulary whatsoever. Barring the omission of a vocabulary I maintain that it is quite a nice language. It aims at being that sort of a language that, if you add a vocabulary, would be a logically perfect language. Actual languages are not logically perfect in this sense, and they cannot possibly be, if they are to serve the purposes of daily life. A logically perfect language, if it could be constructed, would not only be intolerably prolix, but, as regards its vocabulary, would be very largely private to one speaker. That is to say, all the names that it would use would be private to that speaker and could not enter into the language of another speaker. It could not use proper names for Socrates or Piccadilly or Rumania for the reasons which I went into earlier in the lec-

ture. Altogether you would find that it would be a very inconvenient language indeed. That is one reason why logic is so very backward as a science, because the needs of logic are so extraordinarily different from the needs of daily life. One wants a language in both, and unfortunately it is logic that has to give way, not daily life. I shall, however, assume that we have constructed a logically perfect language, and that we are going on state occasions to use it, and I will now come back to the question which I intended to start with, namely, the analysis of facts.

The simplest imaginable facts are those which consist in the possession of a quality by some particular thing. Such facts, say, as "This is white." They have to be taken in a very sophisticated sense. I do not want you to think about the piece of chalk I am holding, but of what you see when you look at the chalk. If one says, "This is white" it will do for about as simple a fact as you can get hold of. The next simplest would be those in which you have a relation between two facts, such as: "This is to the left of that." Next you come to those where you have a triadic relation between three particulars. (An instance which Royce gives is "A gives B to C.") So you get relations which require as their minimum three terms, those we call triadic relations; and those which require four terms, which we call tetradic, and so on. There you have a whole infinite hierarchy of facts,—facts in which you have a thing and a quality, two things and a relation, three things and a relation, four things and a relation, and so on. That whole hierarchy constitutes what I call *atomic* facts, and they are the simplest sort of fact. You can distinguish among them some simpler than others, because the ones containing a quality are simpler than those in which you have, say, a pentadic relation, and so on. The whole lot of them, taken together, are as facts go very simple, and are what I call

atomic facts. The propositions expressing them are what I call atomic propositions.

In every atomic fact there is one component which is naturally expressed by a verb (or, in the case of quality, it may be expressed by a predicate, by an adjective). This one component is a quality or dyadic or triadic or tetradic ... relation. It would be very convenient, for purposes of talking about these matters, to call a quality a "monadic relation" and I shall do so; it saves a great deal of circumlocution.

In that case you can say that all atomic propositions assert relations of varying orders. Atomic facts contain, besides the relation, the terms of the relation—one term if it is a monadic relation, two if it is dyadic, and so on. These "terms" which come into atomic facts I define as "particulars."

Particulars = terms of relations in atomic facts.

Definition.

That is the definition of particulars, and I want to emphasize it because the definition of a particular is something purely logical. The question whether this or that is a particular, is a question to be decided in terms of that logical definition. In order to understand the definition it is not necessary to know beforehand "This is a particular" or "That is a particular". It remains to be investigated what particulars you can find in the world, if any. The whole question of what particulars you actually find in the real world is a purely empirical one which does not interest the logician as such. The logician as such never gives instances, because it is one of the tests of a logical proposition that you need not know anything whatsoever about the real world in order to understand it.

Passing from atomic facts to atomic propositions, the word expressing a monadic relation or quality is called a

"predicate," and the word expressing a relation of any higher order would generally be a verb, sometimes a single verb, sometimes a whole phrase. At any rate the verb gives the essential nerve, as it were, of the relation. The other words that occur in the atomic propositions, the words that are not the predicate or verb, may be called the subjects of the proposition. There will be one subject in a monadic proposition, two in a dyadic one, and so on. The subjects in a proposition will be the words expressing the terms of the relation which is expressed by the proposition.

The only kind of word that is theoretically capable of standing for a particular is a *proper name,* and the whole matter of proper names is rather curious.

Proper Names = words for particulars.

Definition.

I have put that down although, as far as common language goes, it is obviously false. It is true that if you try to think how you are to talk about particulars, you will see that you cannot ever talk about a particular particular except by means of a proper name. You cannot use general words except by way of description. How are you to express in words an atomic proposition? An atomic proposition is one which does mention actual particulars, not merely describe them but actually name them, and you can only name them by means of names. You can see at once for yourself, therefore, that every other part of speech except proper names is obviously quite incapable of standing for a particular. Yet it does seem a little odd if, having made a dot on the blackboard, I call it "John." You would be surprised, and yet how are you to know otherwise what it is that I am speaking of. If I say, "The dot that is on the right-hand side is white" that is a proposition. If I say "This is white" that is quite a different proposition. "This" will do very well while we are all here and can see

it, but if I wanted to talk about it to-morrow it would be convenient to have christened it and called it "John." There is no other way in which you can mention it. You cannot really mention *it* itself except by means of a name.

What pass for names in language, like "Socrates," "Plato," and so forth, were originally intended to fulfil this function of standing for particulars, and we do accept, in ordinary daily life, as particulars all sorts of things that really are not so. The names that we commonly use, like "Socrates," are really abbreviations for descriptions; not only that, but what they describe are not particulars but complicated systems of classes or series. A name, in the narrow logical sense of a word whose meaning is a particular, can only be applied to a particular with which the speaker is acquainted, because you cannot name anything you are not acquainted with. You remember, when Adam named the beasts, they came before him one by one, and he became acquainted with them and named them. We are not acquainted with Socrates, and therefore cannot name him. When we use the word "Socrates," we are really using a description. Our thought may be rendered by some such phrase as, "The Master of Plato," or "The philosopher who drank the hemlock," or "The person whom logicians assert to be mortal," but we certainly do not use the name as a name in the proper sense of the word.

That makes it very difficult to get any instance of a name at all in the proper strict logical sense of the word. The only words one does use as names in the logical sense are words like "this" or "that." One can use "this" as a name to stand for a particular with which one is acquainted at the moment. We say "This is white." If you agree that "This is white," meaning the "this" that you see, you are using "this" as a proper name. But if you try to apprehend the proposition that I am expressing when I say "This is white," you cannot do it. If you mean this piece

of chalk as a physical object, then you are not using a proper name. It is only when you use "this" quite strictly, to stand for an actual object of sense, that it is really a proper name. And in that it has a very odd property for a proper name, namely that it seldom means the same thing two moments running and does not mean the same thing to the speaker and to the hearer. It is an *ambiguous* proper name, but it is really a proper name all the same, and it is almost the only thing I can think of that is used properly and logically in the sense that I was talking of for a proper name. The importance of proper names, in the sense of which I am talking, is in the sense of logic, not of daily life. You can see why it is that in the logical language set forth in *Principia Mathematica* there are not any names, because there we are not interested in particular particulars but only in general particulars, if I may be allowed such a phrase.

Particulars have this peculiarity, among the sort of objects that you have to take account of in an inventory of the world, that each of them stands entirely alone and is completely self-subsistent. It has that sort of self-subsistence that used to belong to substance, except that it usually only persists through a very short time, so far as our experience goes. That is to say, each particular that there is in the world does not in any way logically depend upon any other particular. Each one might happen to be the whole universe; it is a merely empirical fact that this is not the case. There is no reason why you should not have a universe consisting of one particular and nothing else. That is a peculiarity of particulars. In the same way, in order to understand a name for a particular, the only thing necessary is to be acquainted with that particular. When you are acquainted with that particular, you have a full, adequate, and complete understanding of the name, and no further information is required. No further information

as to the facts that are true of that particular would enable you to have a fuller understanding of the meaning of the name.

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DISCUSSION.

Mr. Carr: You think there are simple facts that are not complex. Are complexes all composed of simples? Are not the simples that go into complexes themselves complex?

Mr. Russell: No facts are simple. As to your second question, that is, of course, a question that might be argued—whether when a thing is complex it is necessary that it should in analysis have constituents that are simple. I think it is perfectly possible to suppose that complex things are capable of analysis *ad infinitum*, and that you never reach the simple. I do not think it is true, but it is a thing that one might argue, certainly. I do myself think that complexes—I do not like to talk of complexes—but that facts are composed of simples, but I admit that that is a difficult argument, and it might be that analysis could go on forever.

Mr. Carr: You do not mean that in calling the thing complex, you have asserted that there really are simples?

Mr. Russell: No, I do not think that is *necessarily* implied.

Mr. Neville: I do not feel clear that the proposition "This is white" is in any case a simpler proposition than the proposition "This and that have the same color."

Mr. Russell: That is one of the things I have not had time for. It may be the same as the proposition "This and that have the same color." It may be that white is defined as the color of "this," or rather that the proposition "This is white" means "This is identical in color with that," the color of "that" being, so to speak, the definition of white. That may be, but there is no special reason to think that it is.

Mr. Neville: Are there any monadic relations which would be better examples?

Mr. Russell: I think not. It is perfectly obvious *a priori* that you can get rid of all monadic relations by that trick. One of the things I was going to say if I had had time was that you can get rid of dyadic and reduce to triadic, and so on. But there is no particular reason to suppose that that is the way the world begins, that it begins with relations of order n instead of relations of order 1. You cannot reduce them downward, but you can reduce them upward.

..... If the proper name of a thing, a "this," varies from instant to instant, how is it possible to make any argument?

Mr. Russell: You can keep "this" going for about a minute or two. I made that dot and talked about it for some little time. I mean it varies often. If you argue quickly, you can get some little way before it is finished. I think things last for a finite time, a matter of some seconds or minutes or whatever it may happen to be.

..... You do not think that air is acting on that and changing it?

Mr. Russell: It does not matter about that if it does not alter its appearance enough for you to have a different sense-datum.

THE WASHINGTON MANUSCRIPT AND THE RESURRECTION IN MARK.

THE discovery of the Washington MS. will materially alter the critical apparatus of the next editor of the Greek New Testament. Soden has not observed the fact that this manuscript contains a reading of primary import, in agreement with the lost manuscripts of Eusebius. Westcott and Hort give ἐλθουσαι as a marginal reading at Mark xvi. 5, but they omit its correlative, ἀκουσασαι, at xvi. 8, a reading already known to John Mill of Oxford, in 1707. Westcott and Hort's oversight was due to the fact that no Greek manuscript, used in 1881, contained the reading (except the ungrammatical ἀκουσαντες of Gregory's No. 565). Moreover, they had also failed to observe that lost manuscripts quoted by Eusebius read ἀκουσασαι:

και ἀκουσασαι ἐφυγον, και οὐδενι οὐδεν ειπον· ἐφοβουντο γαρ. (*Ad Marin. Quaest.* 1.)

The critical apparatus of the future will read thus:

Mark xvi. 5.

Corrupted text: εἰσελθουσαι, from the parallel in Luke.

Primitive text: ἐλθουσαι, with Evv. Matth., Johann., Petri; Codd. B, 127.[1]

[1] In my article in *The Monist*, April, 1917, pp. 173f, I adduced the Gothic version here, on the authority of Tischendorf and Massmann; but a study of the Gothic has convinced me that Soden is right in ignoring it.

Mark xvi. 8.

Corrupted text: ἐξελθουσαι, inserted as a correlative to εἰσελθουσαι.

Primitive text: ἀκουσασαι, with lost manuscripts of Sæcc. II-III, apud Euseb. *ad Marin., Quaest.* 1; Codex W, Sinai Syriac, Armenian, and Sahidic versions.

The Washington MS. exhibits the first stage of corruption:

και ἀκουσασαι ἐξηλθον και ἐφυγον.

The ἐξελθουσαι or ἐξηλθον was then being added before the deletion of the ἀκουσασαι. The Vatican and Sinaitic MSS. betray the last and complete stage of corruption, wherein the ἀκουσασαι is dropped altogether:

και ἐξελθουσαι ἐφυγον ἀπο του μνημειου.

Contrast with this the simplicity of Eusebius's early manuscripts which say nothing about the women going out of the sepulcher (because they had never been in it). The Armenian version omits the ἐξελθουσαι, and the Sinai Syriac omits the ἀπο του μνημειου. Thus do these two ancient witnesses, based upon lost Greek manuscripts of the same age as those used by Eusebius, furnish complete support to the reading of that Father. The translator of the future will end Mark thus:

And when they heard, they fled, and said nothing to any one, for they were afraid of.....

Here endeth the Gospel according to Mark.

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LEIBNIZ AND PASCAL.[1]*

IN the History of Mathematics it is generally stated that the higher analysis took its rise in the method of indivisibles of Cavalieri (1635).[2] This assertion, at least as far as the invention[3] of the algorithm of the higher analysis is concerned, is erroneous. In what follows it will be shown, by argument founded on the work of the French mathematicians of the seventeenth century and on the manuscripts of Leibniz, that Leibniz was led to his invention of the algorithm of the higher analysis by a study of the writings of Pascal, more than by anything else.[4]

With regard to the manuscripts of Leibniz, the first letters of the correspondence between Leibniz and Tschirnhaus are weighty; they contain the further discussion of their joint labor during the time that they lived together in Paris (September, 1675, to November, 1676);[5] it is well known that it was during this time that Leibniz invented the algorithm of the higher analysis. Among these letters, one from Leibniz, not hitherto published, which closes the first part of the correspondence between Leibniz and Tschirnhaus, contains a very detailed statement of the studies of Leibniz during his sojourn in Paris; it is beyond dispute of the utmost importance, since it was written only four years afterward and recalls particulars in a most vivid manner.[6]

Next, we have to consider the works of the French mathematicians about the middle of the seventeenth cen-

* For footnotes of both author and translator see *infra*, pp. 550-560.

tury, especially those of Pascal. We know from the facts of Pascal's life that his father, when he moved to Paris in 1631, joined a circle of mathematicians and physicists,[7] of which the history of science has preserved the names of Mersenne, Roberval, Gassendi, Desargues, de Carcavi, Beaugrand, des Billettes, and others. These were in communication, chiefly through Mersenne, with the mathematicians who did not live in Paris, Descartes, Fermat, and de Sluse; so that about the middle of the seventeenth century all that was best (*die Höhe der*) in the science of mathematics was concentrated in Paris.[8] In this circle Pascal moved, hardly yet out of his boyhood, and excited by his eminent talent astonishment and admiration. As an outstanding characteristic of the works of the mathematicians named above there stood forth the endeavor to abandon the method of Cavalieri as lacking every feature of scientific rigor, and to treat the science according to the methods of the Greek mathematicians.[9] Perhaps the ideas of Kepler, in his *Supplementum Stereometriae Archimedeae*,[10] were of influence, when Roberval and Pascal introduced into geometry the ideas of infinity and the infinitely small.[11]

As for those works of Pascal, which belong to this subject, we must mention in particular the solution of the problems, produced by him in 1658 under the assumed name of Dettonville, on the cycloid. By this, and by the method that he employed, he surpassed all the mathematicians contemporary with him, and he earned for himself the fame of being the greatest geometer of his day.

The investigation of the properties of the cycloid had occupied the attention of the most famous mathematicians of the seventeenth century. It is reported that, earlier than anybody else and indeed before 1599, Galileo had had his attention called to this curve in consequence of his construction of arches for a bridge; he endeavored to find its

area in a mechanical way, by weighing a plate of lead of uniform thickness having the shape of a plane bounded by a cycloid; and he found that it was very nearly three times as great as the area of the generating circle. This result he was unable to confirm theoretically. In 1615, Mersenne had his attention called to the cycloid as generated by a rolling wheel; he spent a great deal of time in investigating the nature of the curve, but without success; so that, in 1643, he corresponded with Roberval concerning the difficulties that he had encountered with respect to the curve. Roberval proved, by the help of the method of Cavalieri as improved by himself, that the area of the cycloid is exactly three times that of the generating circle; furthermore, in 1644, he determined the content of the solids formed by the rotation of the cycloid about its base, about its axis, and about the diameter of the generating circle; also he found the centroid of the area of the cycloid. In consequence of a bodily infirmity that robbed him of his rest at night, Pascal, in order to obtain some distraction from his pain, once more took up the investigation of the cycloid after an interval of fourteen years, in the year 1658. His design was to find the area of any chosen segment of the cycloid, the centroid of such a segment, the volumes of the solids described by such a segment by a rotation round either the ordinate or the abscissa, either by a complete, or a half, or a quarter revolution.[12] Inasmuch as the solutions of the problems hitherto investigated had not been done by any general method, but rather by special artificial ways of procedure, the question was that of specially creating a treatment that was applicable in general. Pascal reverted to the method of Archimedes, for determining the quadrature of the parabola by means of the equilibrium of the lever; he generalized the method,[13] by supposing, instead of geometrical figures, unequal weights not merely at the extremities of the lever (which he follows Archi-

medes in terming *balance*) but also at several different distances from the fulcrum; of these, by means of the Arithmetical Triangle which he had invented,[14] he determined the sum and the center of gravity. On the advice of his friends, Pascal, in June, 1658, under the alias of Dettonville,[15] determined to propose to mathematicians for solution the problems that he had solved. October 1, 1658, was settled as the last day for sending in solutions. Particular cases of the proposed problems were solved by Huygens, de Sluse, and Wren, before the appointed day; but this was not sufficient to meet the requirements of Pascal. At the request of de Carcavi, Pascal made known the abovementioned method for solving such propositions in a long letter, at the beginning of October, 1658,[16] and added thereto three further propositions with respect to the cycloid. In this letter are combined five essays, which prepare the way for the solution of the problems of Pascal.

i. *Traitté des Trilignes et leurs Onglets.*[17]

In this essay, the determination of the content and the centroid of a "triligne" and its "double onglet" is reduced to the sum of the ordinates of the axis or the base in a triligne; also Pascal showed that the determination of the content and the center of gravity of the curved surface of the double onglet could be expressed as the sum of the sines of the axis.[18]

The next essay,

ii. *Propriétés des sommes simples, triangulaires et pyramidales,*

is an appendix to the foregoing. By triangular sum, Pascal meant the sum of a number of magnitudes, each one multiplied in succession by the corresponding number in the natural scale. In the same way, a pyramidal sum denoted the sum of a number of magnitudes, each one in suc-

cession multiplied by the corresponding triangular number.[19]

Then comes,

iii. *Traitté des sinus du quart de Cercle.*

In this, Pascal begins by proving the theorem: "The sum of the sines of any arc of a quadrant of a circle is equal to the product of the part of the base, intercepted between the extremities of the outside sines, multiplied by the radius of the circle." By the help of this theorem, he investigated the sum of the sines of a quadrant of a circle, their squares, their cubes, fourth and higher powers,[20] the sum of the rectangles of each sine of the base into its distance from the axis, the triangular and pyramidal sums of the sines of the base, and so on.

The next essay,

iv. *Traitté des sinus et des arcs de Cercle,*

contains the determination of the sum of all the arcs of a circle measured from the vertex of a quadrant to any ordinate of the axis, the sum of their squares, or their cubes, the corresponding triangular and pyramidal sums, the simple and triangular sums of the sectors, the sum of the solids formed from every sector of a quadrant and the distance of its center of gravity from the base, and so on.

v. *Petit Traitté des solides circulaires.*

In this is investigated the position of the center of gravity of such bodies as are formed by the rotation of half a band of a circle about the axis or base, the sum of the fourth powers of the ordinates of the axis, of their cubes, the position of the center of gravity of the semisolid of revotution arising from a rotation about the axis, and so on.

These five essays conclude with:

UMI

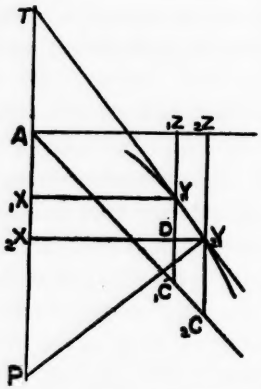
Un Traitté general de la Roulette, contenant la Solution de tous les Problemes touchant la Roulette qu'il avoit proposez publiquement au mois de Juin 1658.

All these works of Pascal are strictly geometrical in treatment, after the manner of the geometry of the ancients; there is not to be found in them a trace of the method of dealing with geometrical problems introduced by Descartes.[21]

It is well known that Leibniz through his acquaintance with Huygens, who lived in Paris from 1666 to 1681, was encouraged to study higher mathematics. More especially, it was Huygens who advised him to read the writings of Pascal. Upon several occasions later, has Leibniz declared, in conformity with this, that he was led to the higher analysis by the study of the writings of Pascal, and thus made his discoveries; first, in the hitherto unpublished letter to Tschirnhaus, of the year 1679, the part of it that relates to our subject being given later; also in a letter to the Marquis de l'Hospital, in the year 1694; further, in a postscript to a letter to Jacob Bernoulli, in the year 1703; and lastly, in the essay, *Historia et Origo calculi differentialis,* written in the last years of his life.

Up to the present time, among the manuscripts of Leibniz there has been found one of great length, that bears the title: *Ex Dettonvillaeno* (?) *seu Pascalii Geometricis excerpta: cum additamentis.* It is not dated; but as it contains work that is in the closest connection with the writings of Pascal to de Carcavi, hence it must be assigned very approximately to the time of his intercourse with Huygens (1673). This cannot be given in its entirety; only the commencement of it follows under the heading III. One special remark has Leibniz made on the five essays which follow Pascal's letter to de Carcavi; he states that the

method of Pascal for determining the surface of the sphere,[22] according to which the surface of a solid formed by the rotation round an axis can be reduced to a plane figure proportional to it, was what induced him to make out a general theorem applicable to all plane figures bounded by a curved line.



The coordinates of $_1Y$ and $_2Y$, two points on the curve, are $_1Y_1Z$, $_1Y_1X$ and $_2Y_2Z$, $_2Y_2X$; $_2YT$ is the tangent at $_2Y$, which is supposed to meet the curve again in $_1Y$, and the normal $_2YP$ is drawn. On account of the similarity of the triangles $_1YD_2Y$and $_2Y_2XP$, we have

$$_2XP \,.\, _1YD = {}_2Y_2X \,.\, _2YD;$$

i. e., the subnormal $_2XP$ applied, at right angles to the axis AX, to the element of the axis $_1X_2X(=_1YD)$, is equal to the ordinate $_2Y_2X$, applied to the element $_2YD$.[23] "But," Leibniz continues, "straight lines which increase from nothing, each multiplied by its corresponding element, form a triangle. For, let AZ be always equal to ZC, and you get the right-angled triangle AZC, which is half the square on AZ, and thus the figure produced by applying the subnormals in order at right angles to the axis is always equal to half the square on the ordinate. Hence, being given a

figure to be squared, that figure is sought whose subnormals are equal to the ordinates of the given figure, and the second figure is the quadratrix of the given figure. Thus from this very simple idea, we have the reduction of surfaces produced by rotation to plane quadratures, and also of the rectification of curves;[24] and at the same time, we can reduce these quadratures to problems of inverse tangents." Thus it came about that Leibniz obtained from this a general method for the quadrature of curves.

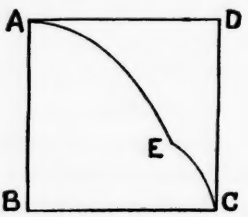
All this was attained to by Leibniz in the first year, 1673/74, of his mathematical studies in regard to the higher analysis. Until this time he had adhered to the rigorous geometrical method, as he found it in the writings of Pascal, in his investigations; acting on the advice of Huygens, he now made himself acquainted with the method of Descartes as being more adapted to computation. The long essay of Leibniz with the title, *Analysis Tetragonistica ex Centrobarycis,* dated Oct. 25, 26, 29, and Nov. 1, 1675, shows clear connection[25] with the above-mentioned method of Pascal; also it shows the improvement that Leibniz had made in consequence of his study of Cartesian geometry, Leibniz commences with Proposition 2 from Pascal's first essay, *Traitté des Trilignes et leurs Onglets,* which he expresses as follows.

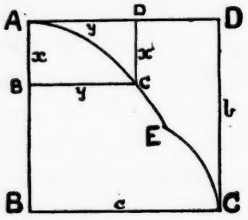
"Let any curve AEC be referred to a right angle BAD; let AB⊓DC⊓a,[26] and let the last x⊓b; also let BC⊓AD ⊓y, and let the last y⊓c. Then it is plain that

$$\text{omn. } \overline{yx} \text{ to } x = \frac{b^2 c}{2} - \text{omn. } \overline{\frac{x^2}{2}} \text{ to } y \ .$$

For, the moment of the space ABCEA about AD is made up of rectangles contained by BC ($=y$) and AB ($=a$);[26] also the moment about AD of the space ADCEA, the complement of the former, is made up of the sum of the squares on DC halved ($=x^2/2$); and if this moment

is taken away from the whole moment of the rectangle ABCD about AD, i. e., from[26] c into omn.x, or from[26] $b^2c/2$, there will remain the moment of the space ABCEA.



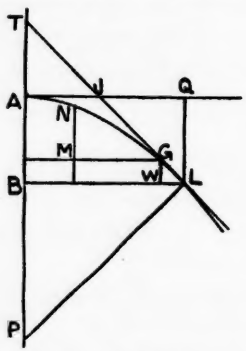


Hence the equation that I gave is obtained; and, by rearranging it, it follows that,

$$\text{omn. } yx \text{ to } x + \text{omn. } x^2/2 \text{ to } y = b^2c/2.$$

In this way we obtain the quadrature of the two joined in one in every case; and this is the fundamental theorem in the center of gravity method."

In the continuation, dated October 29, 1675, in connection with this theorem, Leibniz brings in the characteristic triangle, which has already been mentioned above.



AGL is any curve, BL $= y$, WL $= l$, BP $= p$, AB $= x$, GW $= a$, $y =$ omn. l; hence,

$$\frac{l}{a} = \frac{p}{y} = \frac{p}{\text{omn. } l}, \text{ and therefore } p = \frac{\text{omn. } l}{a} \cdot l.$$

Now, by the theorem given above,[27]

$$\text{omn. } p = \frac{y^2}{2} = \frac{\overline{\text{omn. } l \boxed{2}}}{2} = \frac{\overline{\text{omn. } l^2}}{2} \; ;$$

hence $$\frac{\overline{\text{omn. } l^2}}{2} = \overline{\text{omn. } \overline{\text{omn. } l} . \frac{l}{a}} \; ;$$

"that is," adds Leibniz, "if all the l's are multiplied by their last, and all the other l's again are multiplied by their last, and so on as often as it can be done, the sum of all these will be equal to half the sum of the squares, of which the sides are the sums of these, or all the l's. This is a very fine theorem, and one that is not at all obvious. So is also the theorem,

$$\text{omn. } xl \sqcap x . \text{omn. } l - \text{omn.omn. } l,$$

where l is supposed to be a term of a progression, and x the number which expresses the position or ordinal that corresponds to the l, i. e., x is the ordinal number and l the ordered quantity.

N.B. In these calculations, a law for all things of the same kind may be observed; for, if 'omn.' is prefixed to a number or ratio, or to something indefinitely small,[28] then a line is produced, also if to a line, then a surface, or if to a surface, then a solid; and so on to infinity for higher dimensions.

It will be useful[29] to write $\int$ for 'omn.,' so that

$$\int l = \text{omn. } l, \text{ or the sum of all the } l\text{'s.}$$

Thus, $\int \frac{\overline{l^2}}{2} = \int \overline{\int l} \frac{l}{a}$, and $\int \overline{xl} = x \int \overline{l} - \int \int l$. "

This was the first time that the algorithm for the higher analysis was introduced. In what then follows, Leibniz obtains the first theorems of the integral calculus:

$$\int x = x^2/2, \; \int x^2 = x^3/3,$$

and adds, "All these theorems are true for series in which

the differences of the terms bear to the terms themselves a ratio which is less than any assignable quantity."

Further Leibniz remarks: "These things are new and noteworthy, since they lead to a new kind of calculus. Being given l, and its relation to x, required to find $\int l$. Now this may be obtained by a reverse calculation; thus, if $\int l \doteq ya$, suppose that $l = ya/d$, that is to say, as $\int$ increases the dimensions, so d will diminish them; but $\int$ stands for a sum, and d for a difference.[30] From the given value of y, we can always find y/d or l, or the difference for the y's."

In the investigation that bears the title, *Methodi tangentium inversae exempla,* dated November 11, 1675, Leibniz introduces instead of y/d the notation dy.

Such are the chief points in the story of the introduction of the algorithm of the higher analysis, as far as may be gathered from the extant manuscripts of Leibniz.[31]

In connection with the earlier essay, "Leibniz in London,"[32] I have shown that any influence whatever from external sources upon Leibniz with regard to the introduction of the algorithm of the higher analysis is excluded.

KARL IMMANUEL GERHARDT.

TRANSLATIONS OF THE MANUSCRIPTS

Alluded to by Dr. Gerhardt.

I.

From the letters of Leibniz to Tschirnhaus.

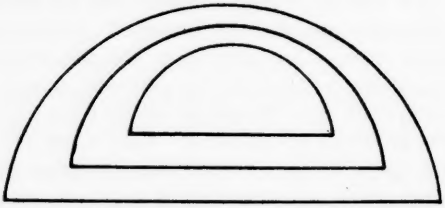
1679.

"You are astonished that Reginaldus[33] should have been able to fall into error over the surface of an elliptic spheroid; but you do not seem to have considered sufficiently how different are the several methods of indivisibles. He certainly understands the Cavalierian method, but that is so circumscribed by narrow limitations that few things of any great importance can be obtained from

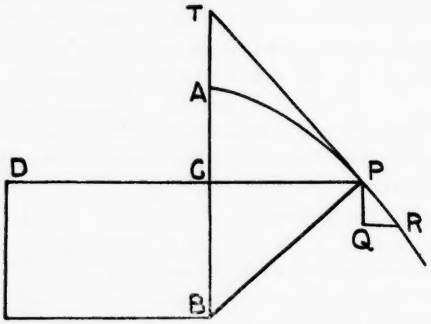
it. There is no doubt that Cavalieri, Torricelli, Roberval, Fermat, and indeed, as far as I know, all the Italian mathematicians were quite unaware of the utility of tangents for the purpose of finding quadratures, or of that which I have been accustomed to call the infinitely small "characteristic triangle" of the figure; indeed, at the present time also in France, I believe that Huygens is the only man that really understands these matters.[34] Pascal himself could not sufficiently express his admiration for the artifice by which Huygens found the surface of the parabolic conoid. Sluse has given no example of these things, by which I am inclined to think that they are unknown to him also. This too is the reason why Huygens and Gregory demonstrated such theorems by roundabout methods, suppressing their analysis, in order not to divulge their method at once so easy and so fruitful.

"The prime occasion from which arose my discovery of the method of the Characteristic Triangle, and other things of the same sort, happened at a time when I had studied geometry for not more than six months. Huygens, as soon as he had published his book on the pendulum, gave me a copy of it; and at that time I was quite ignorant of Cartesian algebra and also of the method of indivisibles,[35] indeed I did not know the correct definition of the center of gravity. For, when by chance I spoke of it to Huygens, I let him know that I thought that a straight line drawn through the center of gravity always cut a figure into two equal parts; since that clearly happened in the case of a square, or a circle, an ellipse, and other figures that have a center of magnitude, I imagined that it was the same for all other figures. Huygens laughed when he heard this, and told me that nothing was further from the truth. So I, excited by this stimulus, began to apply myself to the study of the more intricate geometry, although as a matter of fact I had not at that time really studied the Elements. But I found in practice that one could get on without a knowledge of the Elements, if only one was master of a few propositions. Huygens, who thought me a better geometer than I was, gave me to read[36] the letters of Pascal, published under the name of Dettonville; and from these I gathered the method of indivisibles and centers of gravity, that is to say the well-known methods of Cavalieri and Guldinus. I immediately committed to paper certain things that occurred to me as I read Pascal, of which I now find that some are absurd, others please me very much even at the present time.[37] Amongst other

things, I tried to find a new sort of center. For, I thought that if, to any figure that was given, others that were similar and similarly placed were inscribed, then a "middle point" could be found,[38] at which the figure evanesced, and that being given this point the quadrature could be obtained; later I perceived the difficulty that



made this method ineffective. But to return to the subject, I will tell you how I came to find the method of the Characteristic Triangle. Incidentally Pascal gave a proof of the dimension of the spherical surface proved by Archimedes, that is the moment of a circular curve round the axis,[39] and showed that the radius applied to the axis produced this moment. I, having examined the demonstration with care, observed that, with the aid of the infinitely small characteristic triangle, it was possible to prove the following general proposition for any curve:[40]



"Let AP be any curve and let BP be drawn perpendicular to its tangent AT, to meet the axis in B; then, the ordinate PC being drawn, let the straight line CD be applied to the axis AC, perpendicular to it, and equal to BP. Then if a curve is drawn through all such points as D, we shall have a figure whose area will be the moment of the original curve about the axis, i. e., it will show how

to draw a circle equal in area to that of the surface of a curve rotated round the axis. Since in the circle the straight line BP is always of the same length wherever the point P is taken in the curve, hence the figure produced by the perpendiculars[41] applied to the axis is a rectangle, and thus the surface of the sphere is very easily reduced to a plane area. Now, when from this method I had deduced a general method for the dimensions of such surfaces, I at once took it to Huygens; he was surprised and laughingly confessed that he had made use of precisely the same method for obtaining the surface of the parabolic conoid of revolution. For in that case the curve through every D is a parabola, and hence the figure is capable of quadrature. Since I wished to verify the accuracy of my result in the case of the parabola,[42] I began to look for a method of expressing spaces and curves by reckoning, and then for the first time I really understood those matters of which Descartes wrote. For, previously, I used to calculate in my own way, using not letters but the names of lines. Then, for the first time, I read Descartes and Schooten carefully, acting on the advice of Huygens, who told me that the method of reckoning adopted by these authors was very convenient. Meanwhile having once opened the door provided by the characteristic triangle, I very easily discovered innumerable theorems with which at that time I filled innumerable sheets; but later I found that these had also been noted by Huraet, Gregory, and Barrow.[43] Moreover all these things I came upon in the first year of my apprenticeship to geometry. But after that I struggled forward to far greater things, such as I believe that neither Gregory nor Barrow could ever have reached by their methods, far less Cavalieri or Fermat.[44] About the same time, since I perceived that the finding of quadratures could be reduced to the finding of sums of series, and that the finding of tangents could be reduced to the finding of differences, I put together the fundamental principles of my new calculus,[45] which I call the "differential or tetragonistic calculus," by which I can set with a few little lines those things which could be obtained with great difficulty, if indeed at all, by the help of a mighty apparatus of lines. Moreover I considered in general that the finding of the sum of any series was nothing else but the discovering of some other series, the differences of the terms of which gave the given series, and this other series I used to call the summatrix.[46] The occasion for considering infinite series arose from the work of Wallis and Mercator.[47]

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When I joined their discoveries to mine, I found out new things with no trouble at all.

"At length, when I considered that problems of quadratures might not be of known degree, and yet might be reduced to equations, in which the exponents of the powers were unknowns, a new light dawned upon me and I began to understand that this was something beyond the ordinary analysis, and I called it transcendent, because it employed equations beyond all degrees; and I see that this method, almost alone of its kind, gives a method of determining whether particular problems of this kind are possible or not. Indeed I can easily prove in other ways, and also by the differential calculus more especially, the impossibility of general quadrature of the circle, or that no algebraical line can be given as its quadratrix. What I call algebraical lines are those that Descartes calls geometrical, and by quadratrixes I mean all curves that, being described, will give the quadrature of any portion of a circle whatever. But the manner of finding the impossibility of any particular quadrature, for instance that of the whole circle, is known to me indeed in two ways, the one by the calculus of transcendent exponents, the other by a certain new kind of calculus, embracing all cases, which has not entered the mind of any one before even in his dreams.[48]

"Here you have the story of some of my meditations...."

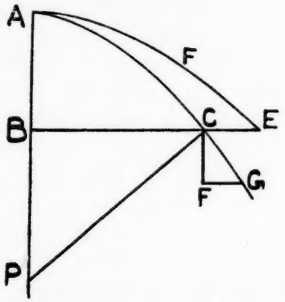
II.

From the correspondence between Leibniz and the Marquis de l'Hospital.

1694.

"I recognize that M. Barrow has advanced considerably, but I can assure you, Sir, that I have derived no assistance for my methods (*pour mes methodes*).[49] At the start I only knew the indivisibles of Cavalieri,[50] and the 'ductions' of Father Gregory St. Vincent, along with the "Synopsis of Geometry" of Father Fabri, and what could be derived from these authors and their like.[51] When M. Huygens lent me the "Letters of Dettonville" (or Pascal), I examined by chance[52] his demonstration of the measurement of the spherical surface, and in it I found an idea that the author had altogether missed; for I remarked that in general, by the same reasoning, the perpendiculars PC, when applied to the axis or set in the position BE, give a line FE, such that the area of the figure

FABEF will furnish a development (*explanation*) of the surface formed by the rotation of AE about AB.



"Huygens was surprised when I told him of this theorem, and confessed to me that it was the very same as he had made use of for the surface of the parabolic conoid. Now, as that made me aware of the use of what I call the "characteristic triangle" CFG, formed from the elements of the coordinates and the curve, I thus found as it were in the twinkling of an eyelid nearly all the theorems that I afterward found in the works of Barrow and Gregory. Up to that time,[53] I was not sufficiently versed in the calculus of Descartes, and as yet did not make use of equations to express the nature of curved lines; but, on the advice of Huygens, I set to work at it, and I was far from sorry that I did so: for it gave me the means almost immediately of finding my differential calculus.[54] This was as follows. I had for some time previously taken a pleasure in finding the sums of series of numbers, and for this I had made use of the well-known theorem, that, in a series decreasing to infinity, the first term is equal to the sum of all the differences. From this I had obtained what I call the "harmonic triangle," as opposed to the "arithmetical triangle" of Pascal; for M. Pascal had shown how one might obtain the sums of the figurate numbers, which arise when finding sums and sums of sums of the natural scale of arithmetical numbers. I on the other hand found that the fractions having figurate numbers for their denominators are the differences and the differences of the differences, etc., of the natural harmonic scale (that is, the fractions 1/1, 1/2, 1/3, 1/4, etc.), and that thus one could give the sums of the series of figurate fractions

$$\frac{1}{1}+\frac{1}{3}+\frac{1}{6}+\frac{1}{10}+\text{etc.}, \qquad \frac{1}{1}+\frac{1}{4}+\frac{1}{10}+\frac{1}{20}+\text{etc.}$$

Recognizing from this the great utility of differences and seeing

that by the calculus of M. Descartes the ordinates of the curve could be expressed numerically, I saw that to find quadratures or the sums of the ordinates was the same thing as to find an ordinate (that of the quadratrix),[55] of which the difference is proportional to the given ordinate. I also recognized almost immediately that to find tangents is nothing else but to find differences (*differentier*), and that to find quadratures is nothing else but to find sums, provided that one supposes that the differences are incomparably small. I saw also that of necessity the differential magnitudes could be freed from (*se trouvent hors de*) the fraction and the root-symbol (*vinculum*), and that thus tangents could be found without getting into difficulties over (*se mettre en peine*) irrationals and fractions.[56] And there you have the story of the origin of my method...."

[At this point Gerhardt quotes his article, *Leibniz in London,* and a long passage from the *Historia,* in corroboration of the foregoing letters. I have omitted them as I have already, in my notes, pointed out the points of resemblance, and the slight differences, between the several accounts that Leibniz gives.—J. M. C.]

III.

Extracts from the geometry of Dettonville or Pascal; with additions.

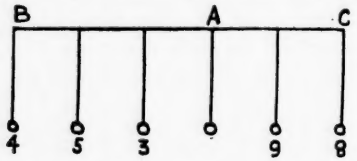
Ca. 1673.

1 2 3 4
A B C D

If A, B, C, D, are quantities, their triangular sum, starting with A, is 1A, 2B, 3C, 4D.

B C D
C D
D

If BC is any straight line divided into any number of equal parts, and any weights, equal or unequal, are suspended at the points of division, and A is supposed to be their point of equilibrium, it is necessary that the triangular sum of the weights on the one arm AB should be equal to the triangular sum of the weights on the other arm AC, where the triangular sum on either side starts from the inner point or from the side A. The reason is that the weights give an effect

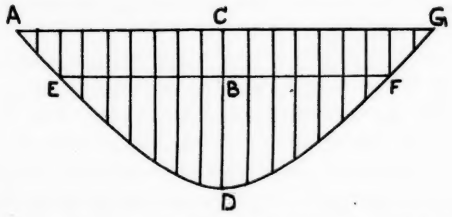


that is compounded of the ratio of the weights and their distances from the center. But these distances, on account of the division

of the straight line or beam of the balance into equal parts increase as 1, 2, 3, etc.

This is what Pascal says; to which I add the following remarks.

Even if the triangular sums on either side of the point are not the same, that is if the two arms are not in equilibrium, yet the moments will always be to one another as the triangular sums, for the moments are always equal to triangular sums.[57] Hence the far more general rule: If any straight line is divided into any number of equal parts, and weighted with any number of weights suspended at the points of division, and if any point of division is taken to be A, then will the moments of the weights on the one arm BA be to the moments of the weights on the other arm CA as the triangular sums starting from that weight which is nearest to A on each side.[58] Also when any figure, i. e., a line, a surface, or a solid, can be put in such a position that a certain line in it can be taken as parallel to the horizon, that straight line can be taken as a balance, and all the points or all the straight lines or all the planes (where the points in the line are assumed to be placed horizontally, or lying in planes of these points set perpendicular to the horizon), may be considered as weights; and thus, if the quantity or progression of these weights is known, and consequently their triangular sum, then the center of gravity of the figure is known; not indeed its position in the figure, but its position in the straight line that has been taken. The center of equilibrium in the figure itself is of this nature: namely, that a straight line passing through it will cut the figure into two parts, such that on each side the triangular sums of the points, straight lines, or horizontals of the solids are equal to one another. Hence the center of gravity of the whole figure being found, the centers of gravity of arms of this kind supposable without the figure may be obtained; for, let the figure be A, and



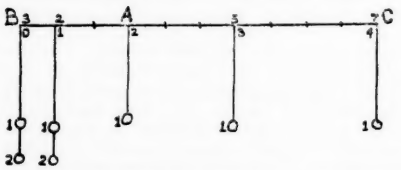
let there be taken a line parallel to the horizon in which is the center of gravity B, and suppose that the center of gravity of it is placed

above a horizontal style or suspended by a thread: then it is plain that the figure will be in equilibrium. But if it is in equilibrium, then the straight line CD, drawn through the center of gravity, will cut the figure in such a fashion that the triangular sums on each side are equal; and if moreover another straight line perpendicular to CD is supposed to be divided into an infinite number of parts by the infinite parallels to CD, the triangular sums of the infinite rectangles on each side will be equal to one another, for by hypothesis the rectangles can be supposed to be suspended as weights from EF as a balance at the points of division (from which it is clear that the suspended weights need not necessarily be understood to be perpendicular to the horizon, but they may be parallel to it). This being the case, the position of the figure may be changed from the horizontal to the perpendicular, and AG become the balance; in which case it is clear that the point of equilibrium will fall at C, since the triangular sums are by hypothesis equal on each side of it. Hence, given the center of gravity of any figure, and assuming a balance either without or within the figure, to which the figure is supposed to be rigidly attached, the point of equilibrium can be found in it, by merely drawing a perpendicular to it through the center of gravity; for this will cut the balance in the point of equilibrium. On the other hand, if the points of equilibrium of two balances for the same figure are given, the center of gravity for the figure can be found (whether it is within or without the given figure; for sometimes the center will fall within the given figure; and sometimes without, as in the case of annular figures, or curved lines, or other incomplete things); that is to say, at the point of intersection of two perpendiculars drawn from those two balances toward the same parts, in the same plane, if the figure is a plane figure, i. e., if the balances are in one and the same plane; but if the two balances are not in the same plane, there is need for three. This is to be investigated.[59]

But the following is a better way: Suppose that the figure is first affixed to one balance, and let the plane through the common perpendicular be the balance and the horizontal be drawn through the point of equilibrium to cut the figure; then let the figure be affixed to another balance, and once more let another plane be drawn to cut the figure; the intersection of these two planes will give a straight line which will contain the center of equilibrium. If now a third balance is taken in addition, or a third plane, the

point of intersection of all the planes, or the point in which the third plane cuts the line already found, will be the center of equilibrium. But if the figures are planes, then two balances and two perpendiculars are sufficient; and also if they are curved lines that lie all in the same plane.

Now it is worth while noting several things in those cases in which the balance is not divided into equal parts; for it may happen that we may know in some way or other the sums of the weights and their progressions, but they are such that, when applied to the balance, they divide it into unequal parts; in that case the progression of the parts into which the balance is divided has to be investigated, as for instance if it is divided into parts that continually increase according to the squares or otherwise. Thus, if we wish to suppose that the weights are equal, while the balance is divided into parts that increase as 1, 2, 3, 4, etc., and yet that this case may come under the rule, we must proceed in this way. Suppose that that point of equilibrium is already found and that it is



2, say; then it is clear that, starting from the point 2 assumed to be the center, the arms should be numbered, and that the point 1 should be marked with the number 2, and the point P with the number 3, and on the other side the point 3 should be marked with the number 3, and the point 4 with the number 7. Now, supposing that the weights are multiplied by the numbers of their own points or arms, it is necessary that the product obtained should be equal;[60] but if it is not, then another point must be sought (or something should be added to, or subtracted from, the weights; for instance, in this case, if the weights are 2, 3 should be supposed to be doubled, or in place of 1,1 we write 2,2 underneath, then there would be an equilibrium on each side, of 10). But to obviate the necessity of going through all the points, a formula should be sought; but if no known progression can be employed for the weights and the parts, a formula will be impossible; but when a known progression can be obtained, then a formula can be found as far as the nature of progression will allow. But the greater part of the difficulty will

vanish in those cases in which the weights can be assumed to be equal. What is more, a very simple general rule has been found which is the reciprocal to that of Pascal, namely, that a point may be assumed such that the triangular sums of the numbers on each arm, always starting from the end and going toward the middle, are equal.....[61]

NOTES.

A. To Dr. Gerhardt's Article.

[1][Translated by J. M. Child from Dr. K. I. Gerhardt's article, "Leibniz und Pascal," in the *Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin*, 1891 (Zweiter Halbband), pp. 1053-1068. All notes added by the translator are put in square brackets.]

[2] "When I speak of the geometry of indivisibles," says Leibniz, "I intend something far more comprehensive than the geometry of Cavalieri, which does not appear to me to be anything but an insignificant (*mediocris*) part of the geometry of Archimedes." [The general statement appears to me to be nearer the truth than that of Gerhardt, who lays unjustifiable stress on the above remark of Leibniz. I have endeavored to show later that there is strong probability that the work of Cavalieri, which Leibniz in the *Historia* acknowledges to have read (see *The Monist*, Oct., 1916, p. 593; also pp. 583, 585 in the same number), was the *Exercitationes Sex*, and not the *Geometria* that was published ten years earlier; perhaps he read them both.]

[3][It seems to me that those who claim merely the symbolism of the Calculus as an "invention" of Leibniz are really detractors from his genius. I have endeavored to show in previous articles in *The Monist* that this symbolism, more especially as regards the sign of differentiation, was a gradual *adaptation and development* of ideas already preconceived for finite differences, until Leibniz had obtained a standardized symbolism for the infinitesimal calculus. This, in my opinion, evidences an immensely greater intellect than that necessary for an "invention"; even if we do take the standpoint that he was helped by the work of his immediate predecessors. Perhaps Gerhardt's word *Erfindung* might be better rendered by "construction" instead of "invention" or "discovery."]

[4][There was absolutely nothing in Pascal to suggest the sign for *differentiation*, and Leibniz might just as easily have obtained his ideas on integration from Galileo or others as from Pascal.]

[5][According to the generally accepted account, Leibniz was in London at the end of the third week of October, 1676, on his way home, via Amsterdam.]

[6][A point therefore to be carefully noticed is that the figure given for the characteristic triangle is totally different from that given in the "Bernoulli postscript" (see *The Monist*, Oct., 1916, p. 585); it is also different from the figure used by him in the manuscript dated Oct. 1674, which is undoubtedly derived from the figure used by Pascal in the opening lemma to the *Traitté des Sinus du quart de Cercle* (see *The Monist*, April, 1917, p. 241, and compare with *The Monist*, Oct., 1916, p. 615); it is different from either of the figures used in the manuscripts of Oct. 29, Nov. 11, 1675 (see *The Monist*, April, 1917, pp. 257, 262, 281), the last of these being like Barrow's Differential Triangle, as used by him throughout his theorems on quadratures. Does this point to a new supposition: namely, that Leibniz originally invented a certain characteristic triangle of his own, essentially different in small detail from that of Pascal, Barrow, or any one else; that then he gradually passed from this to that of Pascal, later to Barrow's form; that he found this the most convenient of all; finally, through lack of memory, he ascribes the earliest form to Pascal, instead

of to himself, making an erroneous apperception of the time at which he had *discovered* this early from? The point is referred to in a later note (40).]

[7] It went by the name of "Compagnie"; out of it grew, in 1666, the "Académie des Sciences."

[8][Gerhardt no doubt here refers to French mathematicians; but the first-mentioned names, of those that lived in Paris, with the exception of Roberval hardly bear comparison with those of the three who did not live there.]

[9] The writings of Roberval and Pascal bearing reference to this have been mentioned in the essay "Leibniz in London." [Unintentionally omitted in the Oct., 1917, *Monist.*]

[10] *Nova Stereometria Doliorum Vinariorum, inprimis Austriaci, figurae omnium aptissimae, et Usus in eo Virgae Cubicae compendiosissimus et plane singularis. Accessit Epitome Stereometriae Archimedeae Supplementum. Lincii an. M DC XV.* See my *Geschichte der Mathematik in Deutschland,* pp. 109ff.

[11] Roberval in a letter to the astronomer Hevelke (Hevelius) in Dantzig, writes: "Concerning analysis, in which I delight, I have far more [theorems]; and no fewer concerning the doctrine of the infinite, which they now call the 'doctrine of indivisibles'...." Published in: *Huygens et Roberval. Documents nouveaux. Par C. Henry*; (Leyden, 1879).

[12][By ordinate and abscissa, Gerhardt means what Pascal calls the axis and base of the segment. Pascal only considered the whole solid of revolution, and the semi-solid, their volumes, their centers of mass, and the centroids of their surfaces; but those for solids generated by a quarter of a revolution could have been deduced quite easily.]

[13][Pascal, in effect, obtained the general formula

$$\overline{x} = \Sigma(mx)/\Sigma(x),$$

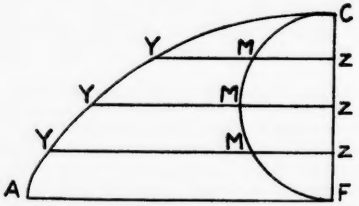
where Σ stands for either a summation of finite quantities, or for the equivalent of integration. If this is to be ascribed to Pascal as an original contribution, then we must assume that he had never seen Cavalieri's *Exercitationes Sex, Exer. quinta,* Theorems 6, 7, 8, and certain others of the fifty propositions that form this section of the book; the section being entirely devoted to centers of gravity, while the method is a direct anticipation of Pascal's.]

[14][What is generally known as the Arithmetical Triangle is not mentioned in the *Lettres de Dettonville;* see Note 19.]

[15][It may be of interest to note that the pseudonym of Amos Dettonville is an anagram on Lovis de Montalte; Lovis, or Louis de Montalte being the pseudonym under which Pascal's *Lettres provinciales* appeared.]

[16] Pascal published what he had written to de Carcavi along with the five essays in the following year, under the title of: *Lettres de A. Dettonville contenant quelques unes de ses Inventions de Geometrie. Scavoir, La Resolution de tous les problemes, touchant la Roulette qu'il avoit proposez publiquement au mois de Juin, 1658. L'Egalité entre les Lignes courbes de toutes sortes de Roulettes et des Lignes Elliptiques. L'Egalité entre les Lignes Spirales et Paraboliques, demonstrée à la maniere des Anciens. La Dimension d'un Solide formé par le moyen d'une Spirale autour d'un Cone. La Dimension et le Centre de Gravité des Triangles Cylindriques. La Dimension et le Centre de Gravité de l'Escalier. Un Traitté des Trilignes et leurs Onglets. Un Traitté des Sinus et des Arcs de Cercle. Un Traitté des Solides Circulaires. A Paris, M DC LIX.* This writing contains the essays of Pascal of the year 1658 together with communications to Huygens, de Sluse, and an unnamed correspondent. From the correspondence of Huygens in the years 1658 and 1659, which is printed in that truly great work: *Oeuvres Complètes de Christiaan Huygens publiées de la Société Hollandaise des Sciences,* we see that a great movement arose among contemporary mathematicians through Pascal's problems, as well as through the printed works that we have mentioned. Leibniz expresses himself thus: "By this time, the controversy [referring to Gregory St. Vincent] had cooled down; when lo! fresh movements in the realm of geometry are stirred up through the whole of France, by Blaise Pascal, a man

of the highest genius, and one who at that time had come nearer to the reputation of Galileo and Descartes than any one else."—This writing of Pascal was recommended for study to Leibniz by Huygens. [As given by Pascal in his letter to de Carcavi, containing the particulars of his method for centers of gravity and the definitions of "trilignes" and "onglets," the problems proposed in June were:

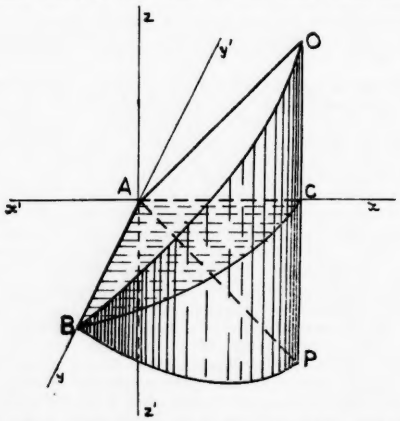


1. To find the dimension and the center of gravity of the space CYZ.
2. To find the dimension and the center of gravity of its semi-solid of rotation about the base ZY, i. e., the solid formed by the triligne CYZ when rotated about the base ZY through half a turn only.
3. To find the dimension and the center of gravity of the solid of revolution about the axis CZ.

To which are added the three proposed in the *Histoire de la Roulette* at the commencement of October:

1. To find the dimension and the center of gravity of the curved line CY.
2. To find the dimension and the center of gravity of the surface of the semi-solid about the base.
3. To find the dimension and the center of gravity of the surface of the semi-solid about the axis.]

[17] By "Triligne" Pascal intends a plane figure bounded by two straight lines perpendicular to one another and a curved line. One of these perpendicular lines is called the axis and the other the base of the figure. If upon such a figure as a base there is erected a right solid, and this solid is cut by a plane which passes through the axis, or the base, then the portion of the

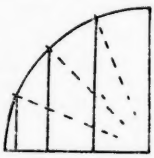


solid that is cut off is called an "onglet." A "double onglet" is obtained if, through the solid formed by production on the other side of the base, there is

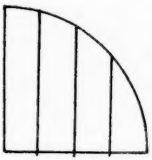
drawn a plane with the same inclination. [The last sentence does not make it clear that the second cutting plane also passes through the axis, or the base, as the case may be; nor that the plane is anticlinic and not parallel to the first plane; nor that Pascal took in general the inclination of the planes to the plane of the triligne to be 45°. I have therefore tried to represent the onglet and the double onglet in a diagram, see above.

ABC is the triligne, OABC is the onglet of (the axis or base) AB, and OBPCA is the double onglet of AB; the angles OAC, PAC are half right angles.]

[18] By *Sinus* Pascal intends the ordinates multiplied by the indefinitely small portions of the arc. [This is a very misleading statement; for Pascal distinctly distinguishes between *sines* and *ordinates*, and thus makes a considerable advance over his contemporaries. He defines them at the same time for finite section and for infinitesimal section; the distinction is made perfectly obvious in a diagram if we use finite section, say, division into four equal parts, of the quadrant of a circle as a special case of a triligne. Now the sum of the



SINES OF THE BASE.



ORDINATES OF THE BASE.

sines or the ordinates are defined as the sum of the rectangles (for, as with all cases of indivisibles, that is what it comes to), formed by the sines or the ordinates respectively multiplied by the corresponding *equal* sectional parts. Thus, to speak of the sum of the sines as being the ordinates multiplied by the small portions of the arcs is quite wrong. Though only in rare cases is the space drawn, Pascal's idea of the sum of the sines is that of the space formed by *straightening* the arc and erecting at each point of division the corresponding sine. Now, as Pascal says in Prop. 1 of the *Traitté des Trilignes*, the sum of the ordinates, which have to be applied to the base, makes the figure itself; while in Prop. 1 of the *Traitté des Sinus du quart de Cercle*, he shows that the sum of the sines (as a special case of the general theorem quoted in iii by Gerhardt *supra*, p. 534) of a quadrant is equal to the square on the radius. Thus, in modern notation,

$$\text{sum of sines} = \int_0^{\frac{\pi}{2}} r \sin\theta \,.\, d(r\theta) = r^2$$

$$\text{sum of ords.} = \int_0^{\frac{\pi}{2}} r \sin\theta \,.\, d(r\cos\theta) = -\tfrac{1}{4}\pi r^2 \,.$$

The concluding paragraph of the *Traitté des Solides Circulaires* runs thus: "All these results arise from the fact that the straight lines OI are ordinates, that is to say that they are equally distant and proceed from equal divisions of the diameter; this brings it about that the simple sum of the ordinates is the same thing as the space intercepted between the extremes. But this is not true for the sines, since the distances between adjacent ones are not equal to one another, and thus the sum of the sines is not equal to the space intercepted between the extremes; *there must be no mistaken idea on this point.*" We find the same care taken by Barrow; but Tacquet breaks down in determining the surface of a cone through not understanding the necessity of this point, and in consequence condemns the method of indivisibles.]

[19][The effect is as Gerhardt states, but these sums are differently *defined* by Pascal in his letter to de Carcavi. The triangular sum of the numbers or magnitudes A, B, C, D, starting with A, (which should be stated), is the sum of all of them, plus the sum of all of them except the first, A, plus the sum of all except the first two, A and B, and so on; this is represented by Pascal as in the margin, and he goes on to show that this is equal to the first taken once, the second twice, and so on. Thus defined, the reason why they are named triangular numbers is obvious. The pyramidal sum is similarly defined as the triangular sum of all, plus the triangular sum of all except the first, plus the triangular sum of all except the first two, and so on. As if there were built up a pyramid having the first triangular sum as its bottom layer, the second triangular sum as the next layer, and so on; thus defined, the origin of the name pyramidal is obvious. Pascal then shows that this is the sum of the quantities taken respectively once, three times, six times, and so on, according to the sequence of the triangular numbers. Then using the property that twice a triangular number diminished by its ordinal number is equal to the square of that ordinal (i. e., $n(n+1)-n=n^2$), he also shows that, if two such pyramidal sums of quantities are taken, and from one of them the bottom layer is removed (i. e., the first triangular sum), then the sum of the two is equal to the sum of the quantities respectively multiplied in succession by the squares of the natural numbers. There is no connection between this and what is usually known as the Arithmetical Triangle of Pascal (see *The Monist,* Oct., 1916, p. 603).]

A	B	C	D
	B	C	D
		C	D
			D

A	B	C	D
1	2	3	4

[20][Pascal simply states the results, as deduced, not from the theorem quoted by Gerhardt, but (together with the theorem quoted) from the preliminary lemma that the radius is to the sine as the hypotenuse of the infinitesimal triangle is to its base: in modern notation, $r : y = ds : dx$, or $r\,dx = y\,ds$, where y is a *sine* and not an *ordinate* in Pascal's sense. All the following theorems are particular cases of the formula $\int y^n\,ds = r.\int y^{n-1}dx$.]

[22] Descartes had spoken disparagingly about Pascal's "Essay on the Conics." Perhaps Pascal's decided opposition to Descartes may be traced back to this. Pascal's niece, Marguerite, writes: "M. Pascal used to speak very little about science; however, when the occasion for doing so occurred, he would state his opinion on those matters about which people were speaking to him. For example, with reference to the philosophy of Descartes, he merely said what he thought. He was of the same opinion as Descartes concerning automatism, but far from being so on the "subtle matter," which he ridiculed. But he could not put up with his (Descartes's) method of explaining the formation of the universe, and he often said: "I cannot pardon Descartes. In the whole of his philosophy, he would have been highly pleased to have dispensed with God; but he could not help making use of him to give a fillip to set the universe in motion. That being done, he had no further use for God." (Fougére, *Lettres, Opuscules et Mémoires de Madame Perier et de Jacqueline, soeurs de Pascal, et de Marguerite Perier, sa nièce.* Paris, 1845, p. 458). [It is more probable that Pascal used geometry, as Barrow did, because he both preferred it and thought it more rigorous than analysis. With regard to the remark on method, Gerhardt does not intend to convey the impression that Pascal abandoned for the more strictly geometrical method of moments the mechanical idea of the *balance,* with which he commences. By the way, to the best of my belief, the word "moment" is never used by Pascal.]

[22][I have gone carefully through the "Lettres of Dettonville," and I find no mention of Archimedes except in one place, namely, Prop. 1 of the *Traitté des Solides Circulaires*; and the whole of this is devoted to *volumes* of solids and their centers. Nor can I find any place where Pascal determines the surface of a sphere, at least not by reducing it to an equivalent plane figure, I have however shown that Barrow does do this (see *The Monist,* Oct., 1916, pp. 610, 611). Surely Leibniz must be confusing the work of Pascal with that of Barrow on quadratures, the latter being so similar to the former in places that Barrow might easily be suspected of "borrowing" from Pascal; much more

easily indeed than Leibniz could be so suspected with regard to either, in spite of his own assertion with regard to Pascal. See Notes 23, 24.]

[23][These are far more like Barrow's results than those of Pascal; while the style is entirely Barrovian and quite different from that of Pascal.]

[24][There is no rectification of curves in Pascal; the whole of this sentence would however serve as a summary of the work of Barrow on rectification.]

[25][Gerhardt states that the Centrobaryc Method, as considered by Leibniz in the manuscripts dated October 25, 26, 29, and November 1, 1675, shows clear connection with the work of Pascal. He asserts that, from a consideration of Archimedes, Pascal was enabled to extend the method of the ancients; he does not seem to be aware of what Cavalieri had done and published as the fifth section of his *Exercitationes Sex*; or else, knowing all about this, he suppresses that knowledge for fear of discrediting the statements of Leibniz concerning the methods of Cavalieri.

The striking points about the work of Cavalieri in question are as follows. He opens by defining gravity as a property of a body, a descensive force. He then defines a heavy body as one possessing this property, and in a note on the definition, he adds that these must be taken to include surfaces, lines, and points. Then he gives the definition of "moment" in its mechanical sense. "The moment of a weight is its endeavor to descend, no matter at what distance it is hung." This is followed by the note: "Since this moment is different at different distances, as will be seen in what follows, it is to be understood from this that the same weight may have different moments." He then defines uniform and uniformly variable (*difformis*) weights, such as a parallelogram in which the density varies as some power of the distance from one side; also he defines the centers of gravity and equilibrium. In Prop. 6 he shows that the moments of bodies are compounded of the ratio of their weights and the ratio of their distances. In Prop. 8 et seq., he combines the doctrine of indivisibles with that of moments to find the centers of gravity of surfaces, chiefly by means of "analogous figures"; thus, a uniform triangle is analogous to a parallelogram whose "difformity is of the first species," i. e., the density varies as the distance from one edge. He shows that, if the difformity is of the nth species, i. e., if the density varies as the nth power of the distance from the edge, then the medial line is divided by the center of gravity into parts in the ratio of 1 to $n+1$, although it is stated rather differently, and only worked out for the first few values; then, using the idea of moments he proceeds from one degree to another in the case of the triangle, where the axis of moments (*limes*) is a parallel to the base through the vertex, and in the following proposition, the base itself; next the semicircle and the hemisphere are dealt with, whether uniform or varying as the distance from the center. In Prop. 36, he lays down the idea that the axis of moments may be outside the figure under consideration; and then proceeds to consider cylinders, cones, parabolic conoids, and the sphere, and truncated portions of them; and finally he finds the moment of a portion of a hyperbola about the asymptote which is not the base of the portion considered. It is interesting to note that Cavalieri, when speaking of the difformity of weight, uses the phrase "*incrementum* difforme gravitatis," i. e., the word *incrementum* is employed to connote a *gradual* increase that follows a definite law. Also it is worthy of remark that he employs the notation, *o. l., o. p., o. q., o. c.,* etc. for "all the lines," "all the planes," "all the squares," "all the cubes," etc.

From the above it will be seen that Cavalieri has given a fairly comprehensive account of the use of moments for the determination of the center of gravity; thus he not only gives far more than Pascal, but anticipates him. Leibniz's matter is far more like that of Cavalieri than that of Pascal; though he seems to be reading Pascal at the time he wrote the third part of the "Analytical Quadrature," by the method of moments, for the last figure in this manuscript (see *The Monist*, April, 1917, p. 268), with the explanatory diagram that I have added on the right of it, is strongly reminiscent of the idea of the onglet of Pascal; although it may have arisen from Cavalieri's work. The great point about this batch of manuscripts of October and November,

1675, is that nearly every figure has the tangent drawn to the curve; now the tangents are never drawn or used either by Cavalieri or by Pascal. A secondary consideration, but still one of importance, is that the subject-matter of these manuscripts is like nothing in Cavalieri or Pascal, as far as the "center of gravity method" is concerned. As we find Pascal's Infinitesimal Triangle idea in the figure of Leibniz's manuscript of October, 1674, I take it that this was the time at which he *finished reading* his Pascal. Hence, I imagine that in October, 1675, he had got a good knowledge of Descartes's algebraical geometry, and began to study Cavalieri's *Exercitationes Sex*; he did not get very far in this before he appreciated the power given by the method of moments; then, probably wearied by Cavalieri's prolix demonstrations, he laid the book aside, and applied Cartesian analysis to the method of moments, running the idea for all it was worth. If this is the case, these manuscripts represent real *original* research, and are not study notes like some of the others.]

[26][The misreadings of Gerhardt, as given in his *Geschichte der höheren Analysis* (see *The Monist*, April, 1917, p. 244) are uncorrected even in 1891, the date of this essay, thirty-six years after the publication of the *Geschichte*! We should have "AB ⊓ DC ⊓ x" and "AB(= x)"—see the figure on the right (p. 538), which is mine while that on the left is the one that Gerhardt gives as that of Leibniz; again Gerhardt's "id est ac in omn.x, sive $a(cb^2/2)$," which makes Leibniz write nonsense, should be "id est a c in omn.x, sive a $cb^2/2$," the "a" being the preposition "away from" and not the length of a line; thus corrected we not only have a sensible reading but the whole paragraph is correct; I have made the correction when translating. Also with regard to Gerhardt's statement that Leibniz starts from an alternative rendering of Prop. 2 of Pascal's *Traitté des Trilignes*, it is worthy of remark that Pascal's figure is altogether different from that of Leibniz; and this is only natural, because there is *no similarity between the theorems, nor is there any relation between the methods of proof*. Pascal's proof is equivalent to the modern method of a change in the independent variable by a conversion to a double integral followed by a change in the order of integration, and is geometrical; that of Leibniz is equivalent to integration by parts, and is merely an example of the theorem of moments.

Thus (Pascal), $\int yx\,dx = \int(\int x\,dx)dy = \int \frac{1}{2}x^2 dy$,

and (Leibniz), $\int yx\,dx = [\frac{1}{2}x^2 y] - \int \frac{1}{2}x^2 dy$;

where Pascal's integrals are taken over the *same* area as one another, and those of Leibniz over *complementary* areas. It seems therefore ridiculous to say that "Leibniz commences with Prop. 2.... which he expresses as follows."]

[27][This means the result obtained geometrically by means of the triangle AZC, in the passage to which Note 23 refers.]

[28][The connection between number, ratio, and *infinitesimal* is peculiar.]

[29][Note the word "useful" (*utile*): the "long s" is introduced merely as a convenient abbreviation in accordance with Leibniz's usual idea of obtaining simplification by means of symbols.]

[30][I have discussed this fully in my translation of Gerhardt's essay, "Leibniz in London" (see *The Monist*, Oct., 1917, p. 545). I have shown there that at least it is highly probable that the d in x/d stands for a certain length, namely the subtangent.]

[31][Note that, in spite of Gerhardt's opening remarks about the algorithm of the calculus being due to reading Pascal, the symbols of integration and differentiation have not been mentioned in anything quoted by Gerhardt in this essay, except in the paragraph just above.]

[32][See *The Monist*, Oct., 1917, where a translation has been given. I believe some of those who read what is there given will, while giving Leibniz full credit for the introduction and *development* of the symbols $\int$ and d, that made the calculus of Leibniz the powerful instrument it was, will find it hard

if not impossible to agree with Gerhardt in his assertion that the ideas of Leibniz were not very strongly influenced by the best points of every single author that he studied, and more especially by the *Lectiones Geometricae* of Barrow and the *Exercitationes Sex* of Cavalieri.]

B. Notes to the Manuscripts of Leibniz.

I.

[33][So far I have failed to find any information as to the error into which Reginaldus fell; he does not appear to be mentioned by either Cantor or Zeuthen.]

[34][The Geometry of Cavalieri is indeed practically all quadratures; but Torricelli himself says (quoted by Tommaso Bonaventura in his preface to an edition of the *Lezione Accademiche*, 1715), in his preface to a *Tract on Proportion*, that he has used indivisibles for tangents as well as for quadratures; Roberval, through his own efforts at concealing his methods, we know comparatively little about; but the germ of Fermat's method is the same as that of Barrow's, namely the Differential Triangle; lastly it is probable that Huygens's knowledge was considerably more than he let anybody know (and so too with Gregory)—cf. Leibniz's words, "suppressing their analysis," a few lines later. It is to be observed that Leibniz deliberately speaks of the mathematicians of France and Italy only; "at the present time," 1679, he must have been aware that Barrow had complete geometrical knowledge, at any rate, of *all* the matters in question.]

[35][The *Horologium* was published in March or April, 1673, and the presentation of a copy to Leibniz was undoubtedly made *after* his return from his first visit to London (Cantor says that the dedication was dated March 25, 1673; see Cantor, III, p. 138). Hence, the date at which Leibniz obtained the Characteristic Triangle can be assigned to some time at least not later than the beginning of October, 1673; and therefore the inclusion of this in the manuscript dated Aug., 1673 (see *The Monist*, April, 1917, p. 238), marks the exact date of its discovery.]

[36][In the "Bernoulli postscript" (see *The Monist*, Oct., 1916, p. 584), Leibniz states that "he obtained a Dettonville from Buotius, a Gregory St. Vincent from the Royal Library, and started to study geometry in earnest." In the *Historia* (see *The Monist*, Oct., 1916, p. 595) Leibniz says that, "in order to obtain an insight into the geometry of quadratures he consulted the *Synopsis Geometriae* of Honoratus Fabri, Gregory St. Vincent. and a little book by Dettonville (Pascal)." In his letter to the Marquis de l'Hospital he says, "At the start I only knew the indivisibles of Cavalieri, and the 'ductions' of Father Gregory St. Vincent, along with the 'Synopsis of Geometry' of Father Fabri" (see *supra*, p. 544). I suggest that the correct explanation of these inconsistencies is that he did get the Dettonville from Huygens as stated here, the St. Vincent from the Royal Library, and the work that he obtained from Buotius was the *Exercitationes Sex* of Cavalieri.]

[37][I think the passage throws considerable light on the character of these manuscripts, besides explaining how it was that Leibniz seems to have taken a very long time to study the works of the authors mentioned. I look on these manuscripts, not as "study notes" merely, nor yet as true "research," but as a mixture of each. I suggest that there is quite enough evidence to make it safe to assert that the characteristic of Leibniz's method of study was to read a very small portion of an author at a time, then to break off and follow out the train of ideas suggested to him by the passage to the furthest limit, before proceeding further with his reading; thus he is led to his own *original* developments. For instance, note in the next sentence how he says he "tried to find a *new* sort of center." This is very characteristic; he is not satisfied with merely acquiring knowledge, even at this early stage, but at once seeks to utilize each point, *as he grasps it*, to obtain something new, something

original previously undiscovered. Cf. the study notes on the work of Pascal, given above under III.]

[38][That is, a "homothetic center."]

[39][As I have been unable to find the word "moment" defined, or even mentioned, in any place except in the *Exercitationes Sex* of Cavalieri, I suggest that this is fairly good circumstantial evidence for the reading of this work by Leibniz before he discovered the theorem in question.]

[40][Observe that this is not the figure used in the manuscript of October, 1674 (see *The Monist,* April, 1917, p. 241), the latter being a diagram that one would naturally expect him to have obtained from the figure in the lemma that commences Pascal's *Traitté des Sinus du quart de Cercle* (cf. Note 6); but is a figure such as one would expect Leibniz to abstract from those given by Barrow, either from Lect. XII, prop. 1, 2, 3, or from Lect. XI, Prop. 1 (see *The Monist,* Oct., 1916, p. 610 and p. 616 respectively. In the latter especially we have the right-angled triangle used by Leibniz on page 596, quoted by Gerhardt in the article translated in the present number). I therefore suggest that Leibniz worked at Barrow and Pascal conjointly, and applied Descartes's analysis to their geometrical theorems. If this is not the case, Leibniz was at fault, for Pascal was discussing *sines* and not *ordinates* (see Note 18); i. e., Pascal was integrating with regard to θ and not with regard to x. Observe also that the figure as given is not correct; the rectangle should be that having AC, CD as adjacent sides.]

[41][Note that the area is taken to be produced by the assemblage of lines applied in order, in the true Cavalierian style.]

[42][Query: urged thereto by a question on the part of Huygens, as to whether Leibniz could now find the properties of the auxiliary curve (see *The Monist,* Oct., 1916, "Bernoulli postscript," p. 585).]

[43][This fits in perfectly with my suggestion that Leibniz attacked Barrow's *Lectiones* at several different times. Having, as I think, taken Barrow's advice given in the preface, he sampled the first few propositions of each lecture, and obtained from those of Lect. XI and XII his Characteristic Triangle. This could I think have been definitely settled if Gerhardt had only given the figure used by Leibniz in the manuscript dated August, 1673. Assuming for the time being that my suggestion is correct and that Leibniz is merely confusing the author that he read at this time, I suggest that characteristically he broke off his reading of Barrow, pursued the idea he had obtained, and made out those theorems on quadratures that he speaks of; this so improved his geometry that later he was able to read Barrow thoroughly and appreciate all that was in it, and to find that his theorems had been anticipated. I also suggest that it was on this second or third reading that he came across the theorem that led to his Arithmetical Tetragonism. A fresh reference to Barrow to find if there were any other ideas that he could develop, considerably later, having already found him a mine of information, would then probably be the occasion on which the marginal notes in his own notation were inserted by Leibniz.]

[44][Leibniz seems to have got these men in true perspective, Cavalieri, Fermat, Gregory, and Barrow, as far as the infinitesimal calculus is concerned. But I doubt whether he, even after he came to his fullest appreciation of Barrow's *geometrical theorems,* or indeed any other person except Bernoulli, ever appreciated the real inwardness of these theorems, or that Barrow's tangent problems could be used, in the manner I have shown in the appendix to my Barrow, to draw a tangent to any curve *given by an equation in either Cartesian or polar coordinates.*]

[45][This I take to mean the principle that differentiation and integration are inverse operations; for it is practically certain that in November, 1675, he could not differentiate a product; otherwise, as previously argued, he would

have verified his solution of the unfortunate equation, $x + y^2/2d = a^2/y$, which he gives as

$$(y^2 + x^2)(a^2 - yx) = 2y^2 \overline{\text{Log } y},$$

by differentiation, as he did with a previous solution that did not contain a product.]

[46][From this probably arose the first germ of the idea of the Quadratrix, in the sense used by Leibniz.]

[47][Substitute *Barrow* and Mercator in conjunction, and we have a feasible suggestion for explaining the first method of proof for the Arithmetical Quadrature of the Circle; the method that Leibniz does not seem ever to have divulged.]

[48][It is impossible for me to conjecture exactly which of his ideas is here referred to by Leibniz; for he calls a mere method by the name of "a calculus," and what we should call a dodge for some particular kind of example by the name of "a method." I think it may be possible that the "transmutation of figures" is referred to.]

II.

[49][Notice that Leibniz says that he has not derived any help from Barrow for his *methods* (*je n'ay tiré aucun secours pour mes methodes*). This is less even than he might have said with perfect truth; for the *methods* of Barrow would have been a veritable hindrance to Leibniz's analytical development. Even when using the Differential Triangle method, and literals for the lengths of his lines, the whole of the working is geometrical in the examples of the method given by Barrow, and not analytical.]

[50][See Notes 35, 36.]

[51][Perhaps this is meant to include Barrow.]

[52][Notice the words "by chance" (*par hasard*); these seem to point to a conclusion that Leibniz read the Pascal in a very desultory manner; this conclusion gets corroborated by the extract given by Gerhardt under the heading III. It is worthy of remark that the "by chance," or "incidentally" (as I have rendered Leibniz's word *forte* in the letter to Tschirnhaus), is made to refer to Pascal. *"Forte Pascalius demonstratbat,"* etc., i. e., "Incidentally Pascal was proving," etc. I think it may be asserted that Pascal missed absolutely nothing that was pertinent to *his purpose*; whereas Barrow certainly missed the opportunity of being the discoverer of the series for the inverse tangent, and thereby the quadrature of the circle, by not applying Mercator's method of division and integration to the result of one of his examples of the Differential Triangle method; as also after giving the method of "transmutation of figures" he missed those things to which it led.]

[53][In a manuscript dated October, 1674 (see *The Monist*, April, 1917, p. 240), Leibniz is using x and y for the variable ordinate and abscissa; while in a manuscript dated August, 1673, he considers "the classification of curves laid down by Descartes." In this manuscript, according to Gerhardt, Leibniz has already constructed the "characteristic triangle," but Gerhardt does not give the particular variant that Leibniz uses in this manuscript. I believe that this will prove to be of the Barrow type, when reference can be made to the original; for the title of the manuscript is strongly suggestive of Barrow, being: *Methodus nova investigandi Tangentes....ex datis applicatis,* etc.; and Pascal's work does not mention tangents.]

[54][That is, as the Characteristic Triangle, leading to integrations, is ascribed to the influence of the work of Pascal, so the Differential Calculus is ascribed to the influence of the work of Descartes. Is this the diplomatic characteristic in Leibniz peeping out? He is writing to a Frenchman, and attributes his work to the respective influences of two Frenchmen. Note that

Leibniz goes on to state that the source of inspiration was summation of series by differences, suggesting the origin of the symbol dx.]

[55][In the manuscripts that we have had under consideration, Leibniz does not appear to have made any practical use of the Quadratrix.]

[56][It is precisely this point which formed the really great improvement in the reckoning section of the infinitesimal calculus. It is just this improvement that is due to Leibniz in analysis, and to Barrow in geometry; although Leibniz did not accomplish anything of the kind until 1676 or 1677. Newton's method by means of series for fractions and roots does not bear comparison, let alone the futility of ascribing Leibniz's method to a perusal of Newton's work.]

III.

[57][All that is any good in the following is to be found in Pascal; I think this corroborates the suggestion I have made as to Leibniz's way when studying a book. It looks here as if he had read about twenty pages of Pascal, and about the same number of pages of Cavalieri's section on centers of gravity; moved thereto probably or possibly by Pascal's remark "....the principle of *indivisibles*, which cannot be rejected by any one having pretensions to rank as a geometer." Then he proceeds to work out his own combination of the two ideas, without bothering to see what else either of these authors had to say on the matter.]

[58][Leibniz tacitly assumes that all the points are occupied; this is necessary for the success of the notion of triangular sums.]

[59][Something very like this is indeed investigated fairly thoroughly in a manuscript dated October 25, 1675 (see *The Monist*, April, 1917, p. 245). Hence these extracts from Pascal were certainly made before that time, though probably not long before.]

[60][This is the rendering for "*productum fieri aequale*"; he probably means that what is produced on the one side, i. e., the sum of the moments on one side of A, should be equal to the sum of the moments on the other side. But this endeavor to obtain *something new* seems rather futile.]

[61][It would have been interesting to have seen what this simple rule was. Probably nothing more than the propositions given by Pascal as Prop. 1, 2, 3 of his method of the *balance*; this would corroborate my suggestion that Leibniz did not study Pascal very steadily or thoroughly (cf. Notes 37, 43, 52, 57).]

SUMMING UP.

The notes and criticisms that I have made in these five articles on the manuscripts of Leibniz may give the impression that I am an anti-Leibnizian. This is quite wrong. My prime object was to show, to the best of my power, that the charges of plagiarism brought against Leibniz by partisans of Newton, and indeed by Newton himself in the *Recensio* published in the *Philosophical Transactions*, were unfounded. I considered that the charges in the *Recensio* were perhaps the hardest to be answered, since they were not only direct charges, backed with circum-

stantial evidence, but they were also set forth very cleverly. Also I thought that the method of defense adopted by Gerhardt and other partisans of Leibniz did as much harm to him as the strongest attack of avowed opponents, such as Sloman. If I am anti-anyone, I am anti-Gerhardt. Never surely did any man have such a glorious opportunity as Gerhardt, in the whole history of scientific controversies; surely there never was an advocate who left himself so open to the attacks of the opponents. Gerhardt starts with the theory that every single word of Leibniz represents gospel truth; and that it is almost blasphemy to doubt it; in consequence he is soon in difficulties when it comes to reconciling the varying statements of the sequences of events that are made by Leibniz at different times. But, once the idea is accepted that Leibniz, while perfectly reliable on the general run of events, is unreliable when it comes to unimportant details, and then all difficulty disappears. I therefore set out with the determination to break down, if I could, the credibility of Leibniz as a witness in his own defense, when it came to unimportant details; then to show that he had opportunities for obtaining everything necessary to the development of the Calculus, that he could not be expected to supply for himself by original work, without having need to know anything of the work of Newton; then to show that these sources of information were set out in a form far more suitable to the requirements of Leibniz than the work of Newton; finally, to clinch the matter, that the analogy of Leibniz's work was so close to these sources, that it was idle to suppose that he made use of any other sources. In other words, (i) the *Analysis per aequationes* was unnecessary to Leibniz, (ii) Newton's method of dodging fractions and roots by means of infinite series was clever, but futile for the needs of Leibniz when developing an operational calculus.

The unreliability of Leibniz with regard to details may

be in some measure due to his admittedly bad memory (which is evidenced by his habit of committing everything to writing), and to passage of time. But in a far greater degree it must be ascribed to the circumstances and characteristics of Leibniz. We know that he designed to compile an encyclopedia of *all* science, and for this he considered not at all the nationality or the personality of the discoverer or the author: all he was interested in were the facts or principles discovered.

That he was unreliable with regard to details is proved by the facts I have adduced:

i. the confusion between Mouton and Mercator in the account of the first charge of plagiarism made against him, or rather an assertion that he had been anticipated (see *The Monist,* Oct., 1916, p. 594, and Note 73);

ii. the varied assortment of figures that he gives to illustrate how he found the Characteristic Triangle (see *The Monist,* Oct., 1916, p. 585, and compare them with the figures given in the accounts quoted by Gerhardt in this essay and those published in *The Monist,* Oct., 1917);

iii. the circumstantial detail of the context of the Archimedean measurement of the surface of the sphere being absent from the author he quotes;

iv. the several different accounts of the order in which he obtained his different books for study, and even the persons from whom he obtained them;

v. the error with regard to the time of the presentation of the copy of the *Horologium* (see *The Monist,* Oct., 1916, p. 594, where, in the *Historia,* it is stated that he received it *before* he left for England on his first visit);

vi. the confusion as to the date at which he obtained his Barrow (see *The Monist,* Oct., 1916, p. 586, where, in the Bernoulli postscript, he states that he found the greater part of his theorems anticipated in "Barrow, when his *Lectures* appeared");

and many other things, all unimportant details singly; but, when taken in combination, they show distinctly that we must only take Leibniz's word as accurately describing the *general* course of events.

Another characteristic of Leibniz seems to have been insistent at all times; he burned to distinguish himself as a discoverer of new things. I have suggested that there may have been an ulterior motive to this desire, namely, to get himself taken into the select circle of mathematicians who corresponded with one another. Thus, when he studied an author, and came across some new idea, he would break off his reading to follow that idea to the limit and exhaust all its possibilities, committing his results to writing, whether they were important or not; there is some evidence, too, that while doing this, he would refer to other authors who had discussed the point under consideration, before returning to his reading.

My motive in trying to show that he got everything from Barrow, *except his methods,* was to remove any charge of plagiarism; for, I consider that even if he had merely rewritten Barrow in terms of Descartes, adding his own notation for the sake of convenience, he would still have done a great thing, and would no more have been guilty of plagiarism from either Descartes or Barrow than Stephenson was from Watt, or Parsons from either of these. Leibniz's Calculus was his own, and would have been his own even on the supposition above. Lastly, it was not only more complete than that of Newton, in that it was an operational calculus, though it did perhaps miss the idea of rate; but also from an intellectual standpoint it was greater, in that it was developed, after its first principles were found out, as a practical theory, while Newton's was developed as a mere instrument for his own purposes.

Assuming, then, that Leibniz did not remember, or did not really care, what his text-books were, so long as he

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was not accused of using somebody else's *methods,* I will try and reconstruct the progress of his reading and his discoveries. His text-books were,

i. Lanzius and Clavius in algebra, and Leotaud for geometry, in his early youth; he also looked through, more or less without understanding them, Descartes and Cavalieri's *Geometria Indivisibilibus.*

ii. On his return from London he brought back with him Barrow, some portions of which he had glanced at in London and on his journey; he obtained Pascal, St. Vincent, and Cavalieri's *Exercitationes Sex*, perhaps a little later than the others; besides these, Wallis and Mercator specially.

He read portions of the Barrow afresh, and obtained the Characteristic Triangle, and found his general theorem from this; meanwhile he is also studying Descartes, and we have the materials for the manuscript of August, 1673. Probably he has had a look through Pascal during this time. He remembers the similarity between the complicated diagrams of Barrow and some of those of Pascal, and starts studying the *Traitté des Sinus,* in which he finds the second variant of the differential triangle that appears in the manuscript of October, 1674. Previous to this, however, his attention has been arrested by Barrow's proof of the inverse nature of the operations of finding a tangent and an area, and the analogy between this and sums and differences strikes him. He has also considered the examples on the differential triangle given by Barrow; one of them suggests the method of Mercator to him, he has already got an idea from Wallis of the summation of the several powers of the variable; he applies this to Barrow's expression, equivalent to

$$d(\tan^{-1}x)/dx = 1/(1 + x^2),$$

in modern notation, performs the division as Mercator had

done, and obtains the series for the inverse-tangent by a summation according to Wallis, i. e., practically an integration. This answers the charge made by Newton that somehow or other he got this series from him or James Gregory. In the same way, he thought that he could obtain other series, but later found that it was beyond his power. We find in this manuscript of October, 1674, an attempt to get something out of an analogous series, the logarithmic series, showing that it is very probable that he has been studying Mercator during the interval between August, 1673, and October, 1674. And in the *Historia* he definitely states that he came upon the Arithmetical Tetragonism in 1674; so that I think that I have offered a reasonable suggestion as to the course his studies took so far. Also in the meanwhile he has been doing a lot of work on series, and has invented his Harmonic Triangle. I now suppose that he completes his study of Pascal, is led by a remark in it to study the *Exercitationes Sex* of Cavalieri (he has already got some acquaintance with the *Geometria Indivisibilibus*, read as a youth), he does not find much in that to his liking, except the notion of moments. He breaks off his reading and proceeds to work out an application of Descartes's algebra to this new idea of moments, the result being the manuscripts of October and November, 1675; here he is led on to the introduction of the symbols for summation and differentiation, though as yet applied to series, and sums of powers. The consideration of the Quadratrix leads him to make a further study of Barrow; and he is led to x/d, by a consideration of Barrow's propositions on the inverse nature of the operations of integration and differentiation. This, combined with the analogy to the inverse nature of summations and differences, leads him to search for a reason why x/d should represent a difference such as he has considered to be denoted by dx. This at a later date necessitates the discussion of what the

result of *operating* with d on a product or a quotient will be. Meanwhile the study of Barrow brings him to that proposition which gives the polar differential triangle; in it he perceives at once the method of "transmutation of figures." I now suppose that he appreciates Barrow more fully and begins to apply Cartesian geometry to Barrow's theorems; in a manuscript dated November, 1675, he attacked the problem of tangents, and in connection with it considered the method of Descartes. In the next manuscript that we have, dated June, 1676, he practically obtained the differentiation of the sine and the inverse sine; his figure, if he had given one, would have been the same as that of Barrow for the differentiation of the tangent. In July, 1676, he attacked the inverse-tangent problem, still considering the work of Descartes. I think, however, that his work on Barrow has taken effect, for from now on he includes the differential factor dx under the integral sign. This is the last manuscript before he went to London for the second time.

Thus, I take it that all Leibniz's work is the result of his own original methods on ideas that have been suggested chiefly by two books, those of Barrow and Descartes; at least, everything could have been suggested by these two books alone, except the notion of "moment," which came from Cavalieri. Thus it was unnecessary for him to have known anything about the work of Newton before he went to London for the second time. What he saw there may have had the effect of corroborating his own work; it could have had little other effect. The final polishing of his method I put down to a study of the Differential Triangle method of Barrow, which Leibniz perceived to be powerful, but found distasteful on account of the geometrical nature of the work.

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A BIOLOGIST'S RELIGION.

"All are but parts of one stupendous whole,
Whose body Nature is, and God the soul."

WHEN I first read Darwin's *Origin of Species* it seemed too plausible to be true. Its conclusiveness appeared to shut out all future speculators from speculating in naturalism. Can it be, I asked myself, that this problem of creation, so perplexing for so many centuries, has been practically solved? Logically, in view of the intellectual assurance of previous generations, the solution, somehow, smacked of prematurity.

Subsequent developments justified the doubt. Investigation uncovered some glaring errors in previously plausible assumptions. What had passed as facts proved to have been purely fiction. The veil of speculation covering many natural phenomena was rent, exposing naked mechanisms whose design and capacities fitted no better to an evolutionary doctrine than to a vitalistic one. A complicated and marvelously correlated animated world revealed itself; each revelation becoming an obstacle to, instead of assisting, further speculation.

Vistas are opening up beyond the temporarily fascinating mechanistic reviews which make manifest their shortcomings. We see in them a kernel of truth surrounded by a mass of chaff.

A pragmatic enlargement of viewpoint suggests itself; a viewpoint which should include the possibilities of special

creation, combined with evolution. The present age cannot expect to monopolize the ultimate truth. History declares for the acceptance of a fundamentally mechanistic groundwork for all living things, functioning by grace of some "vitalistic" or external influence.

In other words, every new evidence points to the correctness of science's diagnosis in so far as it is confined to the mere fact of observed evolution or change of form, and as unmistakably indicates a limitation to these changes conditioned by circumstances as yet beyond our understanding. The traditional conception of creation has lost nothing through competition with the mechanistic hypothesis. Rather it has gained. For the mechanistic hypothesis has failed at just those points where failure counts for special creation.

There is special significance in the expressed need by experts in both biology and physics for another Darwin and another Newton to embody newly discovered facts in theories more consistent with observed phenomena than the several diverse hypotheses now tentatively offered.

Selection, under the first general survey of phenomena, had been made the key-note to evolution, but proved to be an inadequate solution. Here we had a wonderful plausible solution of the world riddle all cut out and dried, when facts cropped up to destroy our confidence. A set of investigators, dissatisfied with the offhand explanations of how evolution came about, determined to ascertain the truth for themselves. Unfortunately they discovered too much—for the fair name of evolution. After careful and prolonged examination of the conditions under which selection occurs, observers found that agency did not work out at all in nature as it had been worked out on paper. They tried many cases of selection and found them wanting. The cases investigated offered absolutely no foothold for such a progressive selection as had been pictured.

Nor were the radical evolutionists more happy in another important particular. They originally placed great confidence in the coincidence between phases through which the animal passes in development (as from egg to maturity its form in embryo suggests the fish, bird, etc.), between these phases and the present geographical distribution from old-world primitive types to new-world more complex ones.

Thus it is the eye can pass in review the respective stages through which the primitive animal supposedly gained its monkey-like complexity both by observation of the course of development of higher animals, and by placing the existing animals of the world in a corresponding scale.

The fact that the embryonic form of the highest vertebrate recalls in its earlier stages the first representatives of its type in geological times and its lowest representatives at the present day, speaks only of an ideal relation, existing, not in the things themselves, but in the designing mind. While these transient resemblances of the young among higher animals in one type to the adult condition of the lower animals in the same type, suggest physical continuity, each one of the primary divisions of the animal kingdom is bound to its own form of development, which is absolutely distinct from all others. No type of animal diverges in the slightest degree from its own structural character. The lower animals are never seen to rise a shade beyond the level which is permanent for the group to which it belongs. The higher ones are never seen to stop short of their final aim, either in the mode or the extent of their transformation.

The hopes of the mechanist to read the book of life from the embryonic development of higher animals seems to have come to naught. Wherever practical tests are available the evidence indicates at least its minor inapplicability. Among others Tower found that his modified beetles repre-

sented a process of synthesis rather than of accumulation. The modified beetles skipped stages represented by transitions in their ancestors, and altogether physiologically behaved in a manner to suggest the futility of determining relationship and directions of evolution through life histories of species. This plausible recapitulation theory, so important in the earlier phases of evolutionary doctrine, thus lost much of its importance as a measurer of biological movements.

The mechanistic interpretation carries its own penalty. A study of growth and form undertaken with a view to substantiate the mechanistic claims reveals the fact that in simpler organisms, "whose form is due to the direct action of a particular physical force, similarity of form is not necessarily an indication of phylogenetic relationship."

The appeal of biological formula-making is well nigh irresistible. It is so tempting to reduce biological movements to a definite and precise rule consistent with our conception of its activities. The "Age and Area" formula is a case in point. All the more interesting because it brings into consideration the still disputed flora and fauna of New Zealand. The "Age and Area" formula applied to plants presupposes the area occupied by any given species depends upon the age of that species in that country. The older the species the wider its range. Perfectly simple and effective—if true. The problem, however, is a little more complex and less obvious than that. And the formula, which hinges on the distribution of the New Zealand flora, is found faulty in its fundamental assumption. A large and characteristic element of the New Zealand flora, it seems, entered the islands not from Australia on the west, as the author of *Age and Area* supposes, but from the Antarctic regions to the south.

The factors governing the distribution of animals are

even more complex than those effecting plants. Food supply, rainfall, humidity, wetness or dryness of soil, altitude, atmospheric density, safety of breeding-places, water (to land species), land (to water species), nature and availability of cover, light-intensity, temperature, interspecific pressure, parasitism, and individual or racial preferences, are some of the factors responsible for the distribution of animals.

The mere catalogue of these influences dooms any simple analysis of their effects. The mechanisms of geographical distribution alone indicate a constant process of adjustment. Frontier individuals, those on the margin of the habitat of the species, may not prosper as readily or reproduce as prolifically, as those in the more favorable center regions of the species, but they certainly do not, as a rule, beat themselves to death individually against their limiting barrier, of whatever nature it may be.

The most important factor for one species is likely to be of minor importance for another species. Always a combination of factors is accountable. Then, too, there are indications of influences at work other than strictly physical ones. Side by side with facts, apparently the direct result of physical laws, are other facts, the nature of which points quite otherwise. The fauna of the Arctic and that of the Alps show a direct relation between climatic conditions and animal life. Yet even there where the shades of specific difference between many animals and plants of the same class are so slight as to elude the keenest, we have representative types among both plants and animals as distinct and peculiar as those of widely removed and strongly contrasted climatic conditions. Shall we attribute the similarities and differences alike to physical causes? On mountain heights of equal altitudes, where not only climate, but other physical conditions would suggest a re-

currence of identical animals we do not find the same, but representative types.

It is now admitted that even among unicellular organisms, specific stability is of much wider application than the first loose judgments—under the spell of the evolutionary logic—had persuaded us was the case.

Recognizing the inadequacy of physical explanations, the mechanists by force of their experiments, have switched from these tangible, external influences to internal, intangible ones. They have not quite decided yet whether these internal influences manifest themselves in the form of mutations—large steps, or selection—small steps.

A warm controversy is at present waging between these two schools. The fact that there is a controversy indicates that scientists are still in the dark, still out of harmony with evolutionary causes; and this lack of harmony is characteristic of humanity in its attacks upon all the problems of life. To expect too much of partially applicable principles, to push too far, perfectly legitimate, but limited, formulas is a fatal fault. Evolution has every earmark of being true—up to a certain extent. Scientists can never hope to approximate this extent so long as they are determined not to limit it.

Though the heart can be made to function temporarily outside the organism, this complaisance entails no wholesale organic obligation to the mechanistic dogma with its limitless vistas of restless molecules and chemical affinities. It shadows forth the inevitable precision of the incidental as distinguished from the particular. Respiration and circulation owe their machine-like precision to a conscious inspiration, whose remoteness is a guarantee of individuality. In the cosmic sense respiration is just as consciously performed as though oxygen were hand-pumped into the blood-stream; only the consciousness is mercifully and tactfully activated from a distance. Imagine the labor and

concentration necessary for a person to remember breathing and pumping his blood at regular intervals! The very indefiniteness of the manner of approach of this distant consciousness, and the vagueness as to its point of contact, intrigue us into denying its reality.

The accurate and regular working of the mechanical parts argues a designer more eloquently than tons of logic. The further the investigator goes into the details of the marvels of life processes the further he gets from proving these marvels take issue in the chance arrangement of simple chemicals.

Reviewing Dr. J. P. Lotsy's recent extreme advocay of mechanistic doctrine, Professor Jeffrey remarks, "It would apparently be well for the mechanistic biologists, who swarm at the present time, to admit also their indebtedness to the oldest if not the least dogmatic of the sciences, theology. If they had the grace to do so, their debt would doubtless be to Bishop Butler's famous *Analogy of Religion*. Lotsy's comparison of hybrids with metal ores is on all fours with the well-known Butlerian argument that the human worm will enjoy a future winged state because the lowly caterpillar later becomes the resplendent butterfly. Analogies are interesting but they do not constitute scientific argument, however much they may appeal to the socialistic and half-educated mind. Much of the present-day mechanism has a foundation not more substantial than the resemblance between a butterfly and an angel."

Once embarked in the business of making comparisons to substantiate their hypothesis, the imagination will carry far. Not far enough, however, to overcome the facts uncovered by investigation. Here there is another story to tell.

It is difficult to believe that the known world is merely a huge dice-box from which a capricious fate shakes out

an occasional fortunate combination of materials in the shape of an oyster or an elephant.

Morgan's wonderfully intricate and detailed work analyzing the movement of factors which decide the make-up of the fruit-fly, Drosophila, has given a valuable insight into the mechanism of heredity. It portrays the distribution, through mating, of already existent factors; but it gives us no inkling whatsoever of the creative agency making these factors possible. It increases rather than diminishes, the mystery of creation, by showing how characters move from the fertilized egg to the mature organism with marvelous regularity, yet fails to enlighten as to the designer back of their regularity.

Numerous experiments are in progress to test out various mechanistic phases of life processes. These experiments usually culminate in the same conclusion. The mechanical processes are mechanical. In experiments carried on by other investigators to discover causes which regulate the duration of life in Drosophila, it was found that the termination of the first stage of metamorphosis is determined by the production in the body of certain chemical constituents not before present. Further investigation by temperature-rises decreased the length of life of these flies. Hence it seems probable that longevity is determined by chemical reaction. Then we come to the endless chain which leaves the chemical reaction in mid-air, minus a known cause. Male insects of some species die immediately after mating; the female of some species die immediately after laying their eggs. Between the two are all grades of longevity with all sorts of chemical reactions, inspired by causes equally unknown.

The imperfections in the mechanistic logic are matched by imperfections in the geological record. These imperfections are glossed over by popular paleontologists. Paleontologists are imaginative students, who are more im-

pressed by the dry bones of the past than by the living facts of the present. While they are usually cautious to confine their claims of actual cases of evolution to relatively brief geological periods, the popular scientist does not scruple to extend the scope through all geological history. That is why the popular conception of prehistoric man has to be revised every so often to keep in line with the discovery of each newly dug-up prehistoric skull.

Calculations based on comparatively short periods in the world's history fail to enlighten as to origins. We cannot hope to approach very close to the truth if we are content to judge the whole from a small part. The Tertiary age presents but a fraction of the world's geological history. Beyond stretch centuries of great biological activity, of whose trend and products we are now afforded only occasional fossiliferous glimpses.

The moral for snap judgments in the matter is furnished by the findings of the seven blind men of Hindustan who went out to investigate the nature of an elephant; one of whom came in contact with the tail and declared the elephant to be like a rope, another bumped into the leg and was convinced of the elephant's likeness to a tree, and so on.

Sudden physical changes in the earth's surface, at widely separated intervals, were accompanied by important alterations in the organic world. Marked and violent changes in the earth-crust caused new elevations, and at the same time terminated the existing animate creation, introducing new populations entirely different from the preceding one.

These cataclysms offer barriers to physical continuity which no amount of persuasive logic can overcome. Of course the fertile mind can conceive of ways of holding to the idea of progressive and continuous evolution as opposed

to special creation, even in the face of these cataclysms. But there are few real facts to substantiate their claims.

Prof. R. D. Carmichael is authority for the statement that "In the early years of the present century the world of scientific thought has been unexpectedly confronted with a new situation of a rather astonishing sort. Our unquestioning assumption of the continuity of nature appears not to have been well founded."

He demonstrates the likelihood of our being on the verge of interpreting everything in nature as discontinuous. Certainly the concrete evidence indicates the falsity of the continuity principle upon which complete evolution is founded. Students of science, even of the highest ranks, are apt, when drawing to conclusions, to fail to take proper account of altering rates of changes of temperature or pressure. Verified rates for short distances cannot safely be assumed to continue indefinitely without interruption or variation. Helmholtz, the distinguished physicist, limited the earth's atmosphere to twenty-eight kilometers from the surface on a basis of the gradient as then determined. Soundings carried to twice the limit fixed by Helmholtz reveal an interruption of the aero-thermic gradient and entirely upset his calculations of what should be.

Robert Mallet, with his centrum theory of earthquakes, dominated orthodox doctrine among earthquake specialists for full half a century; "and succeeded in keeping seismology out of its rightful field of geology for that period."

Ferrel's predicted whirls about the earth's geographical poles, were proven, by subsequent exploration, to be nonexistent. "Yet so great has been the success of Ferrel's theory as a whole, that despite its contradiction of the facts, the polar calms and whirls are still treated in the latest text-books of meteorology."

On the other hand the continuity theory as applied to evolution in the cosmic sense has been shown also, by in-

vestigation, to have been erroneous. The polar researches of Captain Scott and Sir Ernest Shackleton make manifest the fact of a steady glacial retreat. "The bearing of this conclusion upon the ultimate development of the human race is so far-reaching in its consequences that the great sacrifice attendant upon the prosecution of these researches stands forever as a memorial in the correction of the erroneous and wide-spread conception that the earth is in a period of refrigeration, desiccation, and decay; and establishes the conclusion that it is in the springtime of a new climatic control during which the areas fitted for man's use are being extended and that the moss of polar wastes will be replaced by rye and wheat."

So that either way it is taken the fundamental conception of evolution can find no true basis either in physics or mathematics. The momentum of its original plausibility carried it along past the point where the facts patently discredit it.

We hear echoes of this passing of the critical period by such phrases as "The intellectual bankruptcy of the whole evolutionary theory in the late nineties." The strategetic rescue of the evolutionary theory, and the covering of its defeat from public gaze was most unfortunate for humanity. It gave a false value to the doctrine, "Might makes Right," which many evolutionists are, after the event, in haste to disclaim. Particularly in Germany was the obsession carried over the critical period by the Wallingfordian persuasion of Haeckel. Germany, by a blind adherence and unlimited advocacy of a limited principle, worked itself into a conquesting ecstasy which culminated in the war we are suffering—any one who doubts the connection between the mechanistic conception and the world war has but to read Haeckel and the "Kultur manifestoes" side by side—(see also Northcliffe, *Current Opinion,* Oct., 1917).

The mechanistic conception is a banal attempt to standardize our emotions; but one destined to failure because of the essential falsity of both its premises and conclusions. The philosophy of continuity is the outcome of a misguided hopefulness rather than the result of any positive and convincing inductions.

Deductions from any inadequate basis leave us deep in the mire of metaphysics. True science is never dogmatic and deductive; it is pragmatic and inductive. It is built up slowly from an accretion of tried facts, not suddenly as from the framing of brilliant generalizations. The Darwin theory of mimicry is a case in point. For many years established in the scientific mind by a series of then logical deductions, the whole theory of mimicry and adaptive coloration is now badly in need of revision. Dr. Longley's studies of tropical fishes, and others in similar fields, make clear that the criterion of fitness must derive its sanction from the studied animal's intimate enemies, not from man's conception of what the relation ought to be.

Having been pretty well fed up on the mutual obligations and mechanical fitness existing between bees and flowers, we are somewhat shocked at the introduction of a sordid, stubborn fact into the romance of this interesting relationship. It seems that in the haste of logic-making important considerations were carelessly overlooked — an ant appears in the ointment of perfect argument.

The theory of this scientific-romantic ordered relationship is that the flower, in response to the demand of the bee for nectar, developed the nectar-generating habit; while the bee, reciprocating in response to the demand of the flower for cross-fertilization, a perfecting of the system for insuring its best reproduction, developed features of assistance in carrying ripe pollen from the male organs of one flower to the female organs of another.

This might account for nectar in flowers, but how about

the extra-floral nectaries, nectar-sacs on leaf branches and in other discouragingly irregular places? Contrary to the earlier superficial expectation that such extra-floral nectaries might divert the attention of ants from the greater treasures of the flowers, it appears that plants having these sacs, sometimes have their flowers more robbed by ants than would probably be the case if they lacked extra-floral nectaries. On the other hand bees are like to visit these irregular sweets and neglect their duties to flowers. In such cases the flower may fail to be cross-pollenated, indicating a distinct disadvantage. So that, whatever way this romance in mechanistics is viewed, something must be sacrificed. Either we must sacrifice the fundamental principle upon which the extreme evolutionist insisted, that "no structure can survive unless it is of use," or we must sacrifice the picturesque reciprocity of the bee.

Having proceeded beyond the simplicity of the Darwinian formula, the up-to-date evolutionist willingly sacrifices the older, narrow view, which fastens a use to every character, to the newer attitude looking to haphazard internal influences. External tangible physical influences failing in their obligations, it was inevitable that internal, intangible influences should have a trial. But in this repudiation of an old love and taking on of a new no additional light is shed on the marvels of coordination. Quite the contrary. As we cut off environmental pressure from initiating variation we depart from the prospect of arriving at a tangible explanation of creation. Saddling responsibility for variation on the germ-plasm of the race submerges in deeper mystery those problems of relationship and cooperation which are manifestly the important ones.

The germ-plasm, ordinarily so inexorable, is now the accepted seat of all organic changes since the first primeval atom. To its idiocyncrasies we owe all beauty in form, expression, and fitness. It is the chemical experimenter

par excellence. Hydrogen and oxygen playfully throw aside their obstancies in its magic stream, depart from their strictly ordered existence, and enter into the carefree pastime of concocting complicated essential compounds whose composition cannot be duplicated by the centuries-learned brain of man.

The elusive quality of what we call life is illustrated by the failure of scientists to reproduce the living plasms synthetically though their constituents are known and assembled in proper quantities. What nature accomplishes accidentally, man, with all his accumulated knowledge and resources cannot accomplish at all. Through laborious and long-continued effort he can achieve the lifeless replica: the optical form, such as exists in nature, significantly evades his every effort. The validity of the mechanistic doctrine as an explanation of creation is seriously hampered by his inability to react the drama of his own making.

It is now scientifically admitted that we do not know what protoplasm is. "We have analyzed the substance chemically, we have carefully examined and tried (but without complete success) to describe its structure. We know it is more than merely a chemical compound. It is a historical substance. A watch as such is not." (Dr. C. S. Gager, *The Fundamentals of Botany.*)

There seems to be a fatal shortcoming somewhere in the offhand reasoning relegating the organism to the category of fortuitous chanceling carelessly drawn by the hand of fate from out the immensity of the cosmic reservoir.

That the unpremeditated experiments of a bit of protoplasm in chemical affinities would finally culminate after eons in a scampering monkey is difficult enough of belief; but that this same line of individual experimentation could accomplish all the wonderful collective fitness and coordination of star and sun, water and earth, tree and shrub,

insect and man, is more mythological than the mythiest myth, save for the order of mind which has long accustomed itself to spurn other than a mechanistic explanation however strained. There are, undoubtedly, certain orders of mind so firmly locked in the embrace of conventionality that they cannot break loose. For science, conventionality demands adherence to the mechanistic doctrine, and the conventional appetite is easily satisfied. Any formula composed according to its strict rules is acceptable. Thus we find it eagerly espousing the cause of a definite theory of the universe while the fundamental atom is still an enigma. From this easy habit of accepting piecemeal the mental product of recognized authorities we perceive an historic exhibition of favor and disfavor which does small credit to our decree of finality.

To Democritus and his disciples the world appeared to have been the result of a fortuitous concourse of atoms. Plato and his school declared for the orderly course of nature as due to a divine plan. Descartes advocated an earth formed by the aggregation of puny particles of matter which have an inherent whirling motion. Laplace further enlarged upon this view which was received for several generations without reservation. Serious defects later developed, however, and within the last few years astronomers and geologists perceived its coming discardment. The Planetesimal Theory of Chamberlain saves the remnant of Laplace's Nebular Hypothesis, by enlarging upon additional phenomena which required explaining. When these revising explanations—made necessary by hitherto unconsidered phenomena—become too cumbersome and complicated for further logical acceptance we may expect their total breakdown and a return to older views.

It is ever the way of humanity to deal in extremes. Pragmatism is the most difficult form of philosophy for it to adopt. No half-way measures are satisfactory. Either

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truth must be presented concretely or they will have none of it. Either the mechanism is all or the spirit is all. The possibility of a mechanism divinely planned and ordered, a mechanism which is the highest expression of a designing influence, seems not to have occurred to any number of thinking men.

Yet it is just this possibility which keeps the theorist hovering from pole to pole to discover a principle whose real roots are at the equator.

The unit character conception—whereby the organism was supposed to be a compound or mosaic of characters each one definitely represented in the egg—was most attractive. Evidence of certain characters which do not follow the indicated biologic law, made its modification inevitable. Experimenters soon came to see, particularly in regard to color inheritance, that the matter is most complex. In numerous cases of color inheritance there is little to warrant the assumption that these phenomena are based upon representative particles or individualized entities, and very much to warrant the belief that they are the product of a modified chromogen base, a modified enzym or rather vague capacity for carrying on the process.

All roads lead to Rome. From a simplicity of organization which the mechanistic doctrine demanded, there has gradually developed a recognition of complexity involving a "vitalistic" or external influence, a return to the Platonian viewpoint. Having run the gamut of creatorial guesses and pretty well exhaused the visible supply of possibilities, the theoriest is likely ultimately to double back disappointedly to his starting-point. His intellectual pilgrimage has led into no thoroughfare. Obliged by the obvious shortcomings of his directing instruments to retrace his steps, what more sensible than that he should endeavor to stand once more on the threshold and take stock of the faults and realities of his philosophy? To ascertain the

limits of his intellect and to gage its value as opposed to more subtle and settled intuitions?

The crux of the whole matter is here: whether we feel safer in trusting to the pronouncements of biased observers, or in being guided by finer intuitions. Since the intellect is only equipped to grapple with the things of the intellect, we cannot rightly expect it to do justice to itself in matters entirely outside its domain. Those of simple faith, unhampered by intellectual trappings, unaffected by dictation of historically authorized formulas, are best fitted to appreciate a process having nothing in common with a restricted human intellect.

The untutored savage is less handicapped than the most erudite scholar, more thinly insulated from the simplicity of the creatorial influence. The eyes of the Indian and trapper can detect signs and tokens unseen by the educated white man; their ears can hear rumblings to which the latter is deaf; they have advantages of perceptions which the higher civilization dulls. Time has made it manifest that the intellect is a poor gage for the creatorial plan, primarily because the conscious mental processes are unable to cope with the intangible—the tangible alone is their province.

Nor are the intuitive activities confined to an appreciation of spiritual truths; they enter more largely into worldly affairs than intellectuals are inclined to admit. Mr. Harvey O'Higgins, who has made an extended and intimate study of subconscious activity, provides concrete evidence of the working of the subconscious in practical men like Judge Lindsey and Detective Burns (*Saturday Evening Post,* Oct., 1917). Granting, as we must, that such men have been actuated to their best efforts through the instrumentality of the subconscious we can no longer question its judgments. Then also we are obliged to place a higher value on the intuitive findings of a Fabre than on the purely

intellectual calculations of a Dr. Jacques Loeb or a Haeckel. "What is true of the subconscious mind in artists is almost equally evident in the achievements and careers of many men of great intellect outside the arts. In their biographies, again and again, you will find that the deep secret of their success, the real heart of their mystery, is a gift, an intuition, an instinct that cannot be explained—that is to say a subconscious faculty."

In proportion as men of intellect have been willing to submit to the still small voice of intuition are they successful in a dual world. For where there are two equally important processes in operation each must receive its due share of recognition. Spirit and flesh are the irreconcilable components of duality. The spirit speaks a language quite different and distinct from that of the flesh—no messages from one to the other are translatable. Denial of duality is the easiest way out, but it is a way which leads into endless philosophic sophistries having as their object the confusion of terms, and involves comparisons more pleasing to the imagination than complimentary to the understanding.

For those who see merit in the pragmatic attitude it is possible to differentiate between the strictly mechanistic doctrine and a modified evolution. It is not necessary for them to be either atheistic or egotistic. Limiting God's province carries neither conviction nor appeal. An orderly plan and a variable organism, if viewed from a sufficiently high plain, lose their apparent antagonism, and become merged in one stupendous harmonious whole. No biologist need now fear betraying a trust in admitting "vitalism" or design in the scheme of nature, because he is merely submitting to the dictates of a saner science.

The fact that increasing thousands of intelligent people are eagerly espousing the Christian Science doctrine of the unsubstantiality of matter looks hopeful. The fact also

registers the ordinary course of human reactions. From the extreme of an unsatisfactory materialism to a pure spiritism was an inevitable step. Yet it is clear that life, without matter, would be anchorless, chaotic; matter without life, a superfluous, taskless anchor.

Why should there be any difficulty about granting to Cæsar what belongs to Cæsar, and setting aside for Titania that which is rightfully hers? Only the disinclination of an enlightened people to forego a supposed advantage. Preferring to hasten from one pole of the truth to another, these uneasy persons never pause long enough at the equator to take exact observations. Either their practical minds will accept no compromise, or their idealism will permit no taint.

It is doubtless difficult for some orders of minds to keep separate their business and religious convictions. For the biologist the separation is particularly desirable. Research work is necessarily limited to mechanistic processes of life. But this is not equivalent to denying the existence of a "vitalistic" side. Among the unprejudiced initiated the existence of a ruling intelligence is becoming more and more an admitted possibility. With a further swing of the pendulum we can look forward to a freer, franker, less limited recognition of the power and goodness of God.

> "Up from Earth's Centre through the Seventh Gate
> I rose, and on the Throne of Saturn sate,
> And many a Knot unravel'd by the Road;
> But not the Master-knot of Human Fate."

WALTER SONNEBERG.

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THE GENESIS OF AN ELECTRO-MAGNETIC FIELD.

IN the development of a branch of mathematical physics the first stage often consists of a study of the permanent states of a system, for instance the states of equilibrium, states of steady motion, and states of periodic motion; sometimes the development does not proceed much further than this, but frequently the oscillations about the permanent states are considered in full detail. In later stages of development efforts are made to elucidate the way in which the permanent states are attained, to find the conditions that they should be attained, and so forth. In chemical dynamics and in the theory of the conduction of heat a state of equilibrium is generally approached gradually in one direction without over-reaching the mark, while in ordinary dynamics and in the theory of electricity a permanent state is generally attained as a result of a series of damped oscillations.

In nearly every case in which the approach to a permanent state has been discussed, the system under consideration is supposed to be started with an initial motion, and indeed this seems to be necessary, for instance the transition from one permanent state to another could not otherwise be made, while it frequently happens that a given initial motion would have arisen in the natural (or continuous) order of events from motion of a violent character.

In the theory of electromagnetism the discontinuities

which render possible the transition from one permanent state to another are propagated as waves of discontinuity. The theory of such waves has been developed by many writers, and Prof. A. E. H. Love has worked out the details in the case of the transition from the electrostatic field of an electric doublet to the periodic electromagnetic field of a vibrating electric doublet, considering also the case in which the vibrations are damped. In this and in many other investigations in electromagnetic theory an electrostatic field is regarded as the simplest initial field, and this is generally imagined to fill the whole of space and to have existed for ever. A field which fills the whole of space seems, however, to require the existence of an infinite ether or medium to support it, and as the idea of an infinite medium is repugnant to some minds, it may be worth while to consider the question whether an electrostatic field, which does not fill the whole of space, but is bounded by a moving surface of discontinuity, can arise from a state of affairs in which there is initially no electromagnetic field at all.

An answer to this question may be derived from a careful study of the different solutions of Maxwell's equations for the propagation of electric waves. These equations when written in the symmetrical form adopted by Hertz and Heaviside are as follows:

$$c \text{ curl } \mathrm{H} = \delta\mathrm{E}/\delta t, \quad \text{div } \mathrm{E} = 0,$$
$$c \text{ curl } \mathrm{E} = -\delta\mathrm{H}/\delta t, \quad \text{div } \mathrm{H} = 0,$$

where E and H are the electric and magnetic intensities and c the velocity of light.

In a type of solution which we shall regard as fundamental the complex vector $\mathrm{H} + i\mathrm{E}$ is of the form $\mathrm{M} = mf(\alpha, \beta)$ where the vector m depends on both position and time, while f is an arbitrary scalar function of two quantities α and β which are functions of both position and time. Quantities α and β cannot both be real; they have constant values for certain points, which move along straight lines

with the velocity of light, and which may be conveniently called "light-particle." The fact that the vector $mf(\alpha, \beta)$ provides us with a solution of the equations, whatever the arbitrary function f may be, suggests that the elements of disturbance associated with the different light-particles can be regarded as independent of one another and this is just what a further study of the above solution indicates.[1] It appears in fact that the collection of light-particles which lie at any instant within a small volume carry with them a certain amount of energy which remains unaltered during their motion.

The electric and magnetic intensities E and H are, moreover, at right angles to the direction of motion of the light-particles at a point and so the flow of energy, as indicated by Poynting's vector, is in the direction of motion of the light-particle. It should be mentioned that E and H are also perpendicular to one another and equal in magnitude so that the field is a "self-conjugate" or simple radiant field in which there is a simple propagation of energy but no accumulation or expenditure of energy at any ordinary point of space. In such a radiant field the energy may, perhaps, be regarded as energy of motion and as analogous to kinetic energy, although the view is unorthodox.

Now mathematicians have thought for a long time that all energy is really kinetic. The idea that potential energy can be regarded as kinetic energy of concealed cyclic motion was put forward in 1888 by Sir J. J. Thomson in his remarkable book *The Applications of Dynamics to Physics and Chemistry* and was adopted independently by Hertz in his work on the principles of mechanics.

Sir Joseph Thomson says: "This view which regards all potential energy as really kinetic has the advantage of keeping before us the idea that it is one of the objects of

[1] It is indicated to some extent by the general theory of the characteristics and bi-characteristics of linear partial differential equations. Cf. Hadamard, *Propagation des Ondes.*

Physical Science to explain natural phenomena by means of the properties of matter in motion. When we have done this we have got a complete physical explanation of any phenomenon and any further explanation must be rather metaphysical than physical. It is not so, however, when we explain the phenomenon as due to changes in the potential energy of the system; for potential energy cannot be said, in the strict sense of the term, to explain anything. It does little more than embody the results of experiments in a form suitable for mathematical investigations."

Since the energy in an electrostatic field is generally regarded as potential energy, it is clear that an electrostatic field ought not to be regarded as fundamental in electromagnetic theory, and it is now necessary for us to see if the type of field we have chosen as fundamental fulfils the requirements which Thomson considers as characteristic of a type of motion which can be regarded as fundamental in an attempt to eliminate the idea of potential energy.

In a second passage Thomson says: "As all the energy is kinetic its magnitude remains constant by the principle of the Conservation of Energy, and so the principle of Least Action takes the very simple form that with a given quantity of energy any material system will by its unguided motion go along the path which will take it from one configuration to another in the least possible time." The requirements are evidently fulfilled, and so the next step is to choose α and β so that the radiant field under consideration is of a simple character.

Let S be a point which moves in an arbitrary manner with a velocity less than the velocity of light and let the light-particles start from the different positions of S. If P be an arbitrary point in space there is just one position of S, viz., S_0 from which a light-particle can start so as to reach P at time t. The time at which this particle must leave

S_0 is a suitable value of α for the position P at time t and it is clear that α is constant for all the space-time points covered by the light-particle. To obtain a suitable value of β we must find a complex quantity which will specify the direction of motion of the light-particle. Let a sphere of unit radius be described with S_0 as center and let us imagine a steady irrotational two-dimensional motion of an incompressible fluid on the surface or on a portion of the surface,[2] then the complex variable $\varphi + i\psi$ whose real and imaginary parts are constant along the equipotentials and stream-lines respectively is a suitable value of β. The imaginary fluid motion may, of course, vary in an arbitrary manner as S_0 varies; in other words, β depends on both the time of creation and the direction of motion of the light-particle which arrives at P at time t.

Let us now consider the simple case in which the stream-lines are cut out by planes through two points A and A_0 on the sphere. These lines may be regarded either as lines of electric or magnetic force. In the former case the corresponding radiant field possesses the following characteristics:

The field is produced by the creation at the moving point S of pairs of oppositely electrified light-particles and the rectilinear motion of these charged particles in different directions with the velocity of light. A pair of oppositely electrified light-particles may perhaps be supposed to have been derived from a neutral particle traveling initially with the velocity of light.

This creation of electricity may take place continuously for any length of time, and the rate at which electricity of one sign is produced may either vary in an arbitrary manner or may remain constant, while the directions of rectilinear motion of the charged light-particles may also

[2] In the latter case the electromagnetic field may be limited to a certain portion of space.

vary in an arbitrary manner or remain constant. The electromagnetic field comes into existence as soon as the creation of electrified light-particles begins, it has an outer spherical boundary at any later instant and also has an inner spherical boundary at any time after the creation of the electrified light-particles has ceased.

The type of radiant field which has just been described will be called a field of Heaviside's type because in 1901 Oliver Heaviside[3] gave a particular example in which the point S is stationary and the directions of rectilinear motion of the electrified light-particles are invariable and exactly opposite to one another. In this case the electric force at P at time t is directly proportional to the rate of production of positive electricity at S_0 and inversely proportional to the distance of P from the line of motion of the electrified light-particles produced at S_0. If Q and Q_0 are the positions of these light-particles at time t the electric force at P is tangential to the circle Q_0PQ.

The last construction also applies in the more general case, but to obtain an expression for the electric force the inverse distance from QQ_0 must be replaced by the ratio of QQ_0 to $PQ.PQ_0$ and we must divide by the Doppler factor $1 - v_r/c$, where v_r is the component along S_0P of the velocity of S_0.

In the field which has just been described the electricity is moving with the velocity of light. To obtain a field in which the electricity appears to move with a smaller velocity we must superpose two fields of Heaviside's type in such a way that there is a combination of oppositely electrified charges except on a short interval between the sources of the two radiant fields. Let us first of all consider the case in which the two sources are consecutive and stationary, then the electrified light-particles must travel along the line joining the sources and there is "interference" or an anni-

[3] *Electromagnetic Theory*, Vol. III, p. 122.

hilation of electricity in an arrangement of the following type:

$$\infty \quad - \leftarrow - \; S \rightarrow + \; S' \qquad\qquad \infty'$$
$$+ \leftarrow - \quad - \rightarrow -$$

In this case if the canceling is complete on the lines $^{\infty}S$, $S'^{\infty'}$, we are left with an apparently stationary positive charge on S S′. In the more general case when the sources are moving we can obtain a similar canceling if the lines of motion of the electrified light-particles overlap.

To pass to the case of a moving electric pole we must make the distance S S′ tend to zero while the rate of production of positive electricity increases indefinitely in such a way that its product with S S′ remains finite.

Let us now study the genesis of the field of the stationary electric pole a little more fully. Let us suppose that initially a source S fires out two oppositely electrified light-particles A and B and that when B arrives at a point T very close to A, a source there becomes active and fires out oppositely electrified light-particles some of which just annul B. If these sources remain active and adjust their activities and relative positions so that there is a continual annihilation of electricity outside the interval ST we shall have a constant electric charge associated with an interval ST while an uncompensated charge of the opposite sign is carried by the light-particle A and its immediate successors. Now it is undesirable that an electric charge should travel to infinity, and to avoid this we must suppose that when A arrives at some point U a source there becomes active and fires out light-particles one of which just annuls A and its immediate successors. If the other light-particles then produced are annulled by some of the light particles fired out from a neighboring source V and the two sources U and V maintain the charge UV constant, it is easy to see that this charge must be equal and opposite to that on ST

if no uncompensated electricity is allowed to proceed to infinity.

The genesis of the electromagnetic field of two slowly moving electric poles carrying charges which are equal in magnitude but opposite in sign, proceeds along lines similar to the above. It should be noticed that if each charge varies slightly, perhaps periodically,[4] and the uncompensated electrified light-particles fired out by one are annulled by the other, the two moving poles are joined by two singular moving curves which form the locus of the uncompensated electrified light-particles. The two curves coincide only when the two electric charges are moving in a special manner in a plane or on a hyperboloid of revolution.

It is thought that the uncompensated electrified light-particles may give rise to the phenomenon of gravitation and that the two curves which sometimes join two electric charges of opposite signs may represent singular Faraday tubes which, as Thomson suggests, may be intimately related to the bonds of the chemist.

A theory of gravitation based on these ideas has not yet been formulated in a sufficiently definite form to enable us to make straightforward progress in the solution of the problem of atomic structure, but it seems worth while at present to combine the above results with one or two simple hypotheses of the type usually employed by physicists.

1. We shall assume the existence of discrete electric charges of magnitudes $-e$, $+e$, $+2e$, where e is the elementary quantum of electricity, and shall assume that an electric charge of magnitude $\pm ne$ has $2n$ singular Faraday tubes attached to it. We shall suppose that n of these tubes are described by electrified light-particles moving toward the charge and the other n by electrified light-particles moving away from the charge.

If an atom consists of discrete charges bound together

[4] This seems quite natural if the permanent state has not yet been attained.

by means of their singular Faraday tubes, it is clear that an atom containing more than two elementary charges must either contain at least one charge whose magnitude is greater than e or it must contain a set of charges whose singular Faraday tubes form a closed polygon. The two Faraday tubes issuing from a charge $\pm e$ need not indeed end at the same charge, for it is easy to see that the condition for the canceling of electrified light-particles can be satisfied when the tubes belonging to a number of charges form a closed polygon. We shall find it convenient to assume the existence of both multiple charges and cyclic arrangements of Faraday tubes.

Calling the elementary charge $-e$ an electron, we shall consider elementary nuclei containing a number of elementary positive charges bound together by a number of electrons, the total positive charge being greater than the negative.

2. We shall assume that in a neutral atom the number of electrons outside the elementary nuclei is equal to the atomic number, while the total number of electrons is the number nearest to the atomic weight.

An attempt has been made to build up atoms using the following elementary nuclei:

positive electron	$(1e)$	containing	no electrons
double charge	α $(2e)$	"	two "
triple "	ϑ $(3e)$	"	three "
triple "	φ $(3e)$	"	four "
quintuple "	ψ $(5e)$	"	nine "

It should be remarked that a quintuple can be regarded as built up either from two triple charges of type φ joined by an electron or from two α-particles and a ϑ-particle joined by two electrons. The phenomenon of radio-activity may sometimes be due to the breaking up of an elementary nucleus carrying a quintuple charge, and it is interesting

to note that there are two distinct ways in which this nucleus can break up. The existence of two types of elementary nuclei carrying a triple charge may perhaps account for the existence of isotopes having the same atomic number but different atomic weights.

3. We shall assume that an electron whose two Faraday tubes end on the same elementary nucleus is a valency electron and the number of such electrons is the valency that the atom can exhibit under given conditions. The other electrons outside the elementary nuclei have their Faraday tubes ending on different nuclei and may be supposed to bind them together. The distance between the elementary nuclei may, however, be quite small compared with the distance between an elementary nucleus and an electron. It is clear that an atom built up from a given set of elementary nuclei and electrons can exhibit different valencies and it is quite an interesting mathematical problem to determine the number of different ways in which the parts of our ideal atom can be bound together.

A few examples of atoms built up in the above manner may perhaps suffice.

A carbon atom consisting of two ϑ-particles and six electrons would have an atomic weight 12 and atomic number 6; it would have valency 4 when the two nuclei are bound together by 2 electrons; an odd valency does not seem to be possible.

A cobalt atom consisting of 4 ϑ-particles, 5 φ-particles and 27 electrons would have atomic weight 59 and atomic number 27; it could have various valencies, the numbers 2 and 3 being given by simple arrangements while the number 9 is given by an arrangement in which the 9 nuclei are arranged in a ring, consecutive particles being joined together by two electrons. This fact may be of some interest because in some compounds cobalt appears to have a valency of 9.

A neon atom consisting of two particles, two nuclei with triple charges and ten electrons would have atomic weight 20 or 22 according as the two triple nuclei were of type ϑ or φ. This is of interest in connection with the recent discovery of two types of neon by J. J. Thomson and F. W. Aston.

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CRITICISMS AND DISCUSSIONS.

IS THERE AN INTELLECTUAL CONTENT IN PHILOSOPHY?

[It is with a sense of personal loss that we here chronicle the death of the author of this article, the Rev. Dr. James G. Townsend, of Jamestown, New York, on June 27, 1917. He was born in Pittsburgh in 1839, and once said that he had taken care of himself since he was thirteen, when he lost father, brother, sister and uncle within three days in the plague of cholera which swept Buffalo where the family then lived. He was educated at Oberlin and Allegheny College, working his way through. He enlisted in 1862, and in the battle of Perryville had his left arm so badly crushed that he was never again able to lift his left hand. After his discharge he entered the Methodist ministry where he continued for eighteen years, but then went to Jamestown to found the Independent Congregational Church (now the Unitarian) because he no longer felt in harmony with the Methodist theology and philosophy of life. Four years of active ministry there were followed by a severe illness extending over several years, after which Dr. Townsend founded the First Unitarian Church at Pittsburgh. He spent the last seventeen years of his life in Jamestown, where he kept up his intellectual interests through correspondence with literary friends and contributions to liberal periodicals. The "gospel of beauty" as a moral and educational force was the message of his latest years.—Ed.]

Is there an intellectual content in philosophy? Is there a solid, immutable basis for a metaphysic? Can philosophy give us something real, widen the empire of thought? Can philosophy answer any of those questions which are of supreme importance? For example, can philosophy tell us truths about the subsistence of the universe; truths about this mysterious human soul (whether or not the soul, conscious and individual, survives death); truths about God (for if God be a reality there must be truths about Him); and whether or not this universe is "moving to a good end"?

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Miss Mary W. Calkins, in her recent *History of Philosophy*, says "that the study of metaphysics holds out no promise of definite results." She constantly iterates that philosophy "gives us nothing." Yet despite this denial she asserts that philosophy can tell us whether the ultimate reality "is one or many, spirit or matter." But one wonders why, if philosophy can go this far, it cannot go farther.

Schiller says: "Philosophy makes a difference." May not, then, that "difference" be expressed? It is well for us to be modest, to cry out "*Ignoramus*," to admit our knowledge is but a single leaf, plucked from an interminable forest. But must we yield to those agnostics who cry out "*Ignorabimus*," who aver that we have now gone so far that the last word has been said? Is our passion for the highest truth to be expended in a pursuit that reaches no goal, positive or negative?

Prof. William James, in one of his lectures, said: "In this very university (Harvard) I have heard more than one teacher affirm that 'all the fundamental conceptions of truth have already been discovered by science.'" And Professor James said that to make statements of this kind showed a "lack of imagination," in view of the fact that new conceptions have arisen and new problems have been formulated in our generation. And he intimated that a solution of these problems is not impossible.

But it will be said, if the stupendous labors of the great intellects in the long past, Plato, Aristotle, St. Augustine, Calvin, Spinoza, Kant, Fichte, Hegel, Hume, Mill, Spencer, have brought us nothing, if their strong hands have not been able to lift a single inch that veil of impenetrable mystery which hangs over all, why should we expect to be more successful? There are the ultimate enigmas of existence and there they will remain.

This puts the case very strongly, but is it entirely true? Did not Darwin throw light upon the problem of creation, give us a real advance? And certainly we have, what the thinkers in the past did not have, a wealth of scientific facts and the perfecting of the scientific method.

But it may help us to define, in the beginning of this inquiry, what philosophy is. It is often said that philosophy means to discriminate accurately, to avoid fallacies, collect true premises and deduce just inferences, but I should rather call this logic, not phi-

losophy. Nor would I say it was the province of philosophy to describe phenomena and the facts of experience as exactly, as simply and as completely as possible,—that is science, not philosophy.

In a general way I should say it is the business of philosophy to reveal to us reality, the essential truth, the nature of the universe, the meaning of human life. The precocious young Novalis said that philosophy was "homesickness, a desire to be at home everywhere in the universe." This is certainly fine. At another time he said: "Philosophy bakes no bread, but it has given us God, freedom and immortality." While Novalis had gone too far in assuming that philosophy had solved those great intellectual or metaphysical problems, "God, freedom and immortality," he was not mistaken in the mission of philosophy. It is its province to give us a solution of these riddles of our existence or to throw a great light upon them.

Sir Oliver Lodge, a most cautious thinker, says: "A fair comprehension of the nature of life, and the way it is able to interact with matter, must surely be within our human grasp." But if human intelligence shall pass that mysterious realm which has so long divided life and matter, may not some brave thinker cross the boundary between the visible and invisible worlds and prove that the search for reality is the search for God, is a legitimate search, and that the soul evolved and educated at so much cost, shall not be thrown away at death as so much rubbish? Is it not arrogance, is it not ignorance to say that nothing more is possible to philosophy?

God, freedom and immortality may be possible to a courageous philosophy, with our more exact knowledge of the laws of evolution, and of biology. To set up a fence beyond which thought can never cross is the mark of a commonplace, a timid nature. Has not this fecund universe, which has so much for the hand, for the eye, for the ear, for the heart, something for the mind? Is it not presumption to say that the immense horizons of knowledge, hitherto unknown, can never be opened up?

It will be said in reply to my view of a real intellectual content in philosophy, the assertion of the possibility of arriving at a definite goal in thought, that modern philosophy is distinguished by the emphasis it places upon the "relative" spirit above the "absolute." Modern philosophy says nothing is known or can be rightly known except relatively, or approximately. The sciences of observation, in showing how types of life merge in each other in changes infinitely delicate, have brought about this hesitation. The

faculty for truth is recognized as a power of delineating and putting upon canvas the most delicate and ephemeral shades of thought.

There is a new theory of the intimacy, the relationship of mind and matter, good and bad, freedom and necessity. Hard and technical, or churchly, moralities are giving way to simpler, more charitable views, a recognition of those inevitable strands woven by necessity in our complex lives. Man, in body, mind and soul, is swayed by forces of his present environment, also by influences of heredity and by instincts which strike their roots in the soil of an interminable past. Millions of pulses beat in his mysteriously complex nature.

Now I have no quarrel with this "relative" spirit, no desire to "apprehend the absolute," in those realms in which hard and fast lines are impossible. But I maintain, while recognizing the value of the "relative" spirit, that some things may be exactly and absolutely known. For example, that, conditions being the same, there will always be a certain color and curve to the rose-leaf; that every touch of the world of form, color, and feeling brings to us some contribution, if only we are ready to regard it; that it may not be able to tell what beauty is, but that it ever abides for the delight and refreshment of the human spirit.

The new philosophy may not be able to tell us what the body is, or what the soul is, but it may tell us absolutely that the soul abides after the body has crumbled in death.

The new philosophy may not tell us of the mystery of the being of God, but it may tell us that He cares for the things for which we care, that he hears our human call, and is guiding the world toward happiness and goodness, as James said so bravely.

In this attitude of the new philosophy, assuming that in some lines it may be possible to apprehend absolute truth, it follows the old Greek and Roman teachers like Socrates and Zeno and Seneca, Marcus Aurelius and Epictetus. These teachers sought to elevate men, not by conversion, an appeal to the feelings making a change of will ("for the will they thought was good"), but by education, by the impartation of truth, familiarity with lofty ideals. They said "that to know the truth was to do it," and they were right; for in the end, if you elevate men intellectually, you elevate them morally.

The widening of knowledge, constant association with noble and beautiful ideas, affect character. It is said in reply: "Coleridge

talked like an angel, and did nothing." And men may, in the midst of many opportunities, remain ignorant; but generally, when knowledge is received, when the mind really moves, there is a moral elevation, a higher civilization is created.

In a thoughtful paper entitled "Civilization in Danger," in the latest *Hibbert Journal,* René L. Gérard seems to contend for views directly opposed to those I have been defending. He says "that to believe that philosophic or religious doctrines create morals or civilization is a seductive and fatal error." He maintains that it is not because a people possess noble beliefs, broad and generous ideas, that it is healthy and happy, but rather that, being healthy and happy, it adopts or invents noble beliefs and generous ideas. And further he affirms that a people, by instinct, unconsciously (here he follows Bergson), will draw upon the vast moral and intellectual acquisitions of the past, the rich experience of all the ages, for the beliefs and ideas they need.

There is a profound truth in these suggestions, but is it the whole truth? Professor Gérard admits that noble beliefs and great thoughts are absolutely necessary, that they assure the survival of a people. And he affirms that a healthy people will adopt these ideas "instinctively, unconsciously." They will draw them from the intellectual and spiritual acquisitions of the past, the accumulated experience of the ages. But are not these acquisitions, the great and universal "acceptances" or beliefs, largely the fruit of the gigantic toil of the mighty thinkers of the past, like Isaiah, Socrates, Jesus, Paul, St. Augustine, Luther?

Professor Gérard contends that when the vital instinct of a people is healthy and vigorous it readily suggests to the people the religious and philosophic doctrines it needs to assure its survival. Let us see if this be so. I will take the same illustration that Professor Gérard uses. The barbarians who destroyed the Roman Empire were a people in whom the vital instinct was pure and strong. They were uneducated, but they adopted Christianity, a new religion. And this new religion, with its ideas of brotherhood and forgiveness, was distinctly opposed to their religious ideas of conquest and cruelty. But in receiving or adopting this new religion they were impressed, educated, elevated, (for the Christian missionaries came with the Bible in one hand "and Virgil in the other").

They ascended to a higher level of morals and civilization.

These were the ideas they needed. But if there had been no Christianity, no new religion, no Virgil, would they have invented these ideas, as Professor Gérard says they would? Ideas and beliefs do not grow on the bushes. Is it not most probable that if these new ideas had not been ready for their adoption the barbarians would have remained at the same low level? Without these ideas and teachers there could have been no progress.

Professor Bergson says that the instinct of the hymenopteræ is superior to the intellect of man. Can such a statement be taken seriously? Compare the work of the ants and bees with the proud achievements of the human reason. If instinct is the appropriate organ for apprehending reality, the discovery of truth, why should it be given in such measure to ants and bees, which care not to exercise it, whose range of freedom is so small? How comes it that man, who has a passion for the discovery of truth, has so little of this divine faculty of instinct, the truth-discovering faculty (I mean of course the hard and fast lines of instinct)? Can Bergson or Gérard explain this paradox?

A healthy, vigorous people will have constantly new material, intellectual and spiritual wants or necessities. There must be, then, new and fertile philosophic ideas crystallizing into religious beliefs and ideals. And the sure proof of vitality in a people is the adapting of these new ideas to its new physical, intellectual and spiritual wants.

But unconscious instinct cannot supply these ideas and ideals. Here surely is the inevitable task of the thinker. Professor Gérard even admits that "the role of the conscious reason is, in spite of all, the higher role." We need the inspiration of instinct, of feeling, at times, but we must not forget that the most perfect thing, the most indispensable evolved on this planet is the human intellect.

And when M. Bergson affirms that the sphere of the intellect is "matter" and Professor James, following him, says that its province is "mere surfaces," it seems to me a discrediting of that great faculty which rose to its fulness in Socrates, Isaiah, Jesus, Paul, Pascal, Newton, Darwin. Is not the intellect a metaphysical faculty? And are not its problems metaphysical as well as physical, depths as well as "surfaces"? Show me great art, great music, great poetry, great sculpture that has not the intellectual, the metaphysical strand. Are not the Apollo Belvedere, the Divine Comedy, the Mona Lisa, the C Minor Sonata as well as Newton's laws, in part at least,

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the fruit of intellect? Draw out the intellectual threads, and would you not destroy the integrity of the whole garment?

I think it will be conceded that religion will take a great step forward if philosophy shall touch the ground of reality, if it shall find a true answer to some of those questions which ever press upon the human spirit; if it shall rise above a mere rephrasing of its attitude to a consciousness of our mental demands.

I maintain it is not the function of religion to teach any theory of the world, any truth. This does not mean that truth is indifferent in religion, for that would imply that education and science are valueless. Ignorance makes for poverty and vice. But there is no coming to independent truth or knowledge, truths about *real things,* through a revelation from within, through intuition. Truth is attained only by observation, experiment, analysis, search, the most patient and exact generalization. Man's religion is of the imagination and the heart, but from his *intellect* he receives his truth, from the intellect the heart receives its *light.* Truth is ever a matter of discovery, of science, of philosophy; religion a matter of feeling.

There are our feelings, our common reactions, it will be said, the great beliefs, the universal "acceptances." But our feelings and our instincts, in the end, must wait upon our intelligence. Whenever Christianity ceases to hold the people intellectually, it will cease to hold their hearts. Mr. Balfour has shown, with fine scorn, the cowardice of those who would stand for the dogmas of Christianity, not because they believed they were true, but because to retain them would be better for the morals of the people. That is to say, they would retain Christianity for policy's sake. These men forget that when the more intelligent lose their faith, the multitude will surely, in the end, lose their faith also.

It is not the province of philosophy (no one will say) to strengthen the gross materialism so transcendent about us unless it shall have been first shown true by science. But the tendency of the thought of our greatest men of science is away from the material hypothesis. They no longer believe religion to be a cunning fable devised by king and priest to be an instrument with which they might control the people. Religion is a mighty force and has its roots deep down among the primitive instincts and feelings of the human race. It will be a sad day for humanity when religion shall have become superannuated.

There must be, as Professor Gérard says, the healthy and

beautiful body. There must be intellectual vigor. But there must be, if a people shall endure, the material for intellectual nutriment. There are to-day new intellectual and spiritual wants. There must be new and fertile ideas which may crystallize into new religious beliefs and ideals. To furnish these ideas and ideals is the inevitable task of the poet, the artist and the philosopher.

While Professor Bergson has said many things derogatory of the human intellect, and a lot of nonsense about the original power of the intuitions which we have now lost, he has said many brave words for philosophy. He has made the vital suggestion (which, so far as I have seen, seems to have been unnoticed) that philosophy pursuing certain lines of facts all converging on the same point "may give an accumulation of probabilities which will gradually approximate scientific certainty."

Well, what greater certainty can we ask? And is not the human intellect, with its dogged slave, Observation, its angel-attendant Imagination, practically infinite? May not that intellect which sweeps over infinite time and space, which holds in its hand, like a flower, the whole stellar universe, solve at least some of those problems which are the very cause of philosophy's existence?

Why should we deny the final intelligibility of the universe? Why may not philosophy pass over the threshold of speculation into the domain of actual knowledge? Why may there not be a definite conquest by philosophy as well as by science? Who can limit what philosophy may do when squarely facing the supreme problems and not frittered away, as Bergson says, "upon a host of special problem in psychology, in morals, in logic."

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THE FALLACY IN MR. H. G. WELLS'S "NEW RELIGION."

In his book, *God the Invisible King,* which hails the appearance of a "new religion," Mr. H. G. Wells proclaims himself the spokesman of his age, the "scribe to the spirit of his generation."[1] If he claims to speak for the scientists as well as for the less enlightened portions of society, his conclusions are startling, to say the least,

[1] *God the Invisible King,* p. 171.

in view, not only of the commonly observed lack of religious belief among scientists, but also of the statistical study by which Professor Leuba[2] has shown that the majority of scientists in America, and presumably elsewhere, disclaim any belief in God.

Were it not that he implicates "that very great American, the late William James,"[3] whom he calls his master, Wells's religious views would, perhaps, scarcely merit consideration by philosophers. So far, however, as the views of Wells are due to James's influence, they deserve examination; and the fact that *God the Invisible King* is a book intended primarily for popular consumption need not condemn it in the sight even of professional philosophers, when it is remembered that James (and so why not, perhaps, James's disciple?) could be both popular and profound.

A finite God is proclaimed in Wells's new religion, and at once a point of similarity between Wells and James is noted. James was insistent that the Absolute of the philosophers could not be the God of religion, and Wells is equally insistent upon this point. But, whereas James asserted that personal immortality is the core of religion, and that the chief function of God would be the guaranteeing of immortality,[4] Wells regards this question as an irrelevant issue in religon, interest in which is evidence of egotism.[5] Whether James was not nearer than Wells to a correct interpretation of the religious consciousness regarding the belief in immortality is a question that might appropriately be raised, though I shall omit consideration of it here.

Limiting my discussion to questions of the nature of God, and of the evidence for His existence, in Wells's view, I desire to point out the closeness with which Wells follows James's line of thought, to the extent of committing one of the same fallacies that James commits.

Wells's God is not the Life Force or the Will to Live,[6] neither is He the Collective Mind of the Race.[7] Wells, like James, insists that God must be genuinely personal, existing within a temporal environment, aiding mankind in its upward struggle, and accessible to man through what James calls "prayerful communion". In

[2] J. H. Leuba, *The Belief in God and Immortality*, Boston, 1916.

[3] Wells, *op. cit.*, p. 172.

[4] *The Varieties of Religious Experience*, p. 524; *Human Immortality*, Ingersoll Lecture. This view of James's accounts in part for his interest in psychical research.

[5] *Op. cit.*, Preface, p. xix. [6] *Op. cit.*, p. 17. [7] *Op. cit.*, pp. 61, 62.

Wells's view, as in James's, evidence for God's existence is found in so-called religious experiences, mystical in nature. James expresses it as follows: "There *are* religious experiences of a specific nature ....They point with reasonable probability to the continuity of our consciousness with a wider spiritual environment."[8] "Personal religious experience has its root and center in mystical states of consciousness."[9] And Wells says similarly: "Modern religion bases its knowledge of God and its account of God entirely upon experience."[10] "This cardinal experience is an undoubting, immediate sense of God. It is the attainment of an absolute certainty that one is not alone in oneself.... The moment may come while we are alone in the darkness, under the stars, or while we walk by ourselves or in a crowd, or while we sit and muse. It may come upon the sinking ship or in the tumult of battle.... After it has come our lives are changed, God is with us and there is no more doubt of God. Thereafter one goes about the world like one who was lonely and has found a lover.... One is assured that there is a Power that fights with us against the confusion and evil within us and without."[11] In accepting the mystical experience as the basis of religious belief, Wells agrees completely with James. As Wells himself says,[12] "So far as its psychological phases go the new account of personal salvation....has little to tell that is not already familiar to the reader of William James's *Varieties of Religious Experience.*"

When God's existence is argued for upon the basis of the religious experience, a crucial question arises regarding the externality and objectivity of the God that is believed in. Here arises what I have called the fallacy of false attribution, "which consists in the erroneous interpretation of an experience whereby the experience is attributed to an external, divine source in cases where a physiological explanation is adequate to account for it."[13] With Wells as with James there is no doubt regarding the objectivity of the God evidence for whose existence is thought to be found in mystical experiences. James classifies himself as a "piecemeal supernaturalist."[14] Piecemeal supernaturalism "admits miracles and

[8] *A Pluralistic Universe*, pp. 299, 300.

[9] *The Varieties of Religious Experience*, p. 379.

[10] *Op. cit.*, p. 20. [11] *Ibid.*, pp. 23, 24. [12] *Ibid.*, p. 21.

[13] Cf. the author's article "Two Common Fallacies in the Logic of Religion," *Journal of Philosophy, Psychology, and Scientific Methods*, Vol. XIV, pp. 653-660. The above quotation is from page 657.

[14] *The Varieties of Religious Experience*, p. 520.

providential leadings, and finds no intellectual difficulty in mixing the ideal and the real worlds together by interpolating influences from the ideal region among the forces that causally determine the real world's details."[15] Nowhere in Wells's writings do we find quite so frank a statement of supernaturalism; but in denying that God is the Collective Mind of Humanity,[16] in admitting "help from without,"[17] in speaking of an "exterior reference"[18] of the religious experience, and in insisting that "God is an external reality,"[19] Wells commits himself to such a view. And with such a view goes the fallacy of false attribution, which is found in the arguments of both Wells and James[20] for the existence of God.

The fallacy of false attribution is committed by Wells, we must agree, so far as it is possible to explain the religious experience in terms of physiological psychology; and there seems to be little difficulty in accounting for the experience as a form of emotionalism, which is *interpreted, after* the experience, as an experience of communion with God. The mystic *believes* that he experiences an objective God, a reality which is more permanent than the passing experience, and which is the source of the experience. Such belief is essential in connection with the mystical experience in order to make it a religious experience. But, though the *belief* in God is present, God need not be real; and, in fact, it is the belief, and not the object of the belief, God, that does the "work" in religion that James and Wells speak of. Thus Wells says:[21] "Prayer is a power. Here God can indeed work miracles." And in saying this he is illustrating very clearly the fallacy of false attribution. If a physiologically grounded psychology is to be admitted to the circle of the sciences, then we must say here that it is the psycho-physiological activity of belief that does the "miraculous" work—work which is falsely attributed to God.

Whoever claims that evidence for the existence of God, defined concretely enough to be significant, as Wells's God is defined, is to be found in experiences of a mystical sort, must give reasons for asserting that the mystical experience is anything more than a strongly marked emotional state, in which the sentiment of love is prominent, together with a strong conviction regarding the divine

[15] *Ibid.*, pp. 520, 521. [16] Wells, *op. cit.*, p. 61.

[17] *Ibid.*, p. 26. [18] *Ibid.*, p. 78. [19] *Ibid.*, p. 82.

[20] See the author's article, *loc. cit.*, pp. 657-660, for a discussion of the fallacy of false attribution as found in James's views.

[21] *Op. cit.*, p. 155.

source of the emotion and the divine object of the human sentiment of love. The conviction, or belief, moreover, as to the source of the experience is not derived from the experience, but from tradition, education, and social influences in general. Professor Hocking's claim that "the love of God is the one natural instinct of man"[22] is ungrounded. The biologist and the psychologist fail to discover "love of God" among the instincts. There is no religious instinct. Love of God is a form taken by instinctive love when *interpreted* in a religious fashion; and the religious interpretation is not instinctive, but is due to social influences. The experience comes from "below," through the sublimation of a very primitive instinct, and is to be explained in naturalistic terms; but it is interpreted by the mystic as coming from "above," and not to be explained naturalistically. In the mystic's interpretation there inheres the fallacy of false attribution.

The mystical solution of the problem of religion, to which Wells resorts, is inadequate except under one condition—that mysticism be made a thorough-going metaphysical doctrine, involving the complete denial of any reality to the world that the sciences study. Only in Nirvana could such a doctrine be consistently maintained.

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A PHILOSOPHICAL LITTERATEUR.

Prof. S. P. Sherman's work has been for the last few years one of the features of the New York *Nation.* Memorable are the vivid character-portrayal of Professor Kittredge (issue of September 11, 1913), the rollicking zest of "The Gaiety of Socrates" (July 15, 1915), and the mordant logic of his dissection of Mr. Roosevelt (November 29, 1917). But chiefly as the upholder of the conservative tradition in literary criticism has Mr. Sherman attained distinction. Most of his reviews of this character have recently been issued in a revised and enlarged form, together with an introduction expounding fully though somewhat loosely the author's *Weltanschauung.*

The book, entitled *On Contemporary Literature* (Henry Holt & Co.), makes delightful and stimulating reading. Though the accent is at times academic, the style is often vivid and racy. Take

[22] W. E. Hocking, *The Meaning of God in Human Experience,* p. 577.

this penetrating criticism of Dreiser's realism: "If you expect to gain credence for the notion that your hero can have any woman in Chicago or New York that he puts his paw on, you had probably better lead up to it by a detailed account of the street-railway system in those cities." Or this satirical sketch of Alfred Austin: "While we who live in territorial homes have been asking, *Où sont les neiges d'antan*?—'Where are the wives who sit on the nest and never stir?', he has sung on imperturbably, celebrating the Lucile, the Dora, the Maud of the mid-Victorian dream—the fair and lissome English maiden blushing and trembling toward her lover and her lord with the reverence implanted in her unsunned bosom by God and Nature."

When, however, Mr. Sherman proceeds to apply philosophical standards to the subjects of his criticism, as he does very often, he shows himself as remote from the modern trend, as much the victim of traditions and formulas as the laureate object of his satire. He modestly disclaims any complex philosophical apparatus, but the fact is that he seems not to have any philosophical apparatus at all beyond what might be gleaned from a sedulous study of *belles-lettres*. He appeals "to the general reason and experience of mankind against the conclusions of the ratiocinative faculty of the individual." The superstition of a solid and homogeneous "general reason" can only be held by those who coolly ignore the thought of the East and arbitrarily suppress the heterodox opinion of the West as voiced by Euripides, Lucretius, Gottfried von Strassburg, Voltaire, and Heine. Only within the realm of science, which nevertheless regards the individual's judgment as the final court of appeal, has there grown up enough real unanimity of opinion to justify an appeal to the "general reason." Mr. Sherman seems reluctant to admit the authority of science, sneering, for instance, at Anatole France's acceptance of the scientific probability that there is a limit to the evolution of life on our planet; yet we propose to test his philosophy by a comparison with the only real approximation we have to a "general reason", the consensus of scientific opinion.

Mr. Sherman's view of man is summed up in these sentences: "On the lowest level is the natural world, which is the plane....of the animal passions or affections....On the middle level is the human world....working upon the natural world; but governed by reason, the special human faculty....On the third level is the

spiritual world, which is the plane of spiritual beings and the home of eternal ideas."

How does this analysis tally with the findings of science? Mr. Sherman seems to say that the passions and affections we share with the animals are not a part of our real humanity. Would he have us regard as unhuman the sex instinct, the creative impulses, tribal loyalty, and individual fidelity? He gravely asserts that "it is of the essence of a man to lay down his life out of reverence for his great-grandfather." Yet what is this essence of a man but the Quixotic outgrowth of a filial devotion observable in many animals? Has Mr. Sherman never felt inclined to regard even our noblest instincts as rather typically animal than human, to exclaim with the cynic, "The more I see of men, the more I like dogs?" Elsewhere he remarks that "the impulse to refrain we can find nowhere in nature. It is part of the pattern design of human society that lies in the heart of man." Really it would seem as if he had never seen a squirrel store away a nut, or a trout resist a baited hook, or a mother bird keep a juicy worm for her nestlings. Either the word "refrain" is used in some esoteric sense, or else Mr. Sherman's words are a specious flattery to our nature which it requires no scientist to refute.

Mr. Sherman, however, is not consistent as to what is the vital differentia between man and brute, and perhaps would prefer to have us judge him by his pronouncement that reason (not the impulse to refrain) is "the special human faculty." I wonder if he has consulted the opinions of psychologists on this point? Holmes says: "If we define reason as the derivation of conclusions through the comparison of concepts, it is not improbable that no animal below man employs this faculty. But this is far from implying that animals cannot perform mental operations which are essentially inferential in their nature." Hobhouse declares that, "If we allow reason to the human species in general, and yet restrict it to that species, it must be by identifying the term reason arbitrarily with a certain grade in the development of analysis." Apparently the Jesuit Father Wasmann is almost alone among comparative psychologists in holding the animal and human minds utterly distinct.

Again we must quarrel with Mr. Sherman when he speaks of reason as a power by which the natural world, including our own desires, is "governed." For him the human ideal is attained "when Ariel, the lawless imagination longing for liberty, and Caliban, the

incarnation of the lusts, powers, and instincts of our animal nature ....yield to the wonder-working sway and sovereignty of benignant reason, represented by Prospero." Now reason rules no one: it only points the way to the fulfilment of one's strongest desires. That much neglected essayist, Mr. Bernard Shaw, has put this whole matter with inimitable skill. "The difference between Caliban and Prospero is not that Prospero has killed passion in himself whilst Caliban has yielded to it, but that Prospero is mastered by holier passions than Caliban's." Shaw goes on: "The ingrained habit of thinking of the propensities of which we are ashamed as 'our passions,' and our shame of them and our propensities to noble conduct as a negative and inhibitory department called generally our conscience [Mr. Sherman calls it the reason, Mr. More the inner check], leads us to conclude that to accept the guidance of our passions is to plunge recklessly into the insupportable tedium of what is called a life of pleasure....Reactionists against the almost equally insupportable slavery of what is called a life of duty are nevertheless willing to venture on these terms.... No great harm is done beyond the inevitable and temporary excesses produced by all reactions; for, as I have said, the would-be wicked ones find, when they come to the point, that the indispensable qualification for a wicked life is not freedom but wickedness." Here are words which Mr. Sherman and the philosophers of the humanist school may ponder. At least, they will not retort that Mr. Shaw is a pseudo-scientist, for on this point he has the whole psychological world behind him.

Finally we come to the third story of Mr. Sherman's edifice, the spiritual world, defined as "the plane of spiritual beings and the home of eternal ideas." I fear that science has sadly depopulated this world. As Mr. Sherman knows full well,

> "Al was this land fulfild of fayerye;
> The elf-queen with her joly companye
> Daunced ful ofte in many a grene mede;
> * * *
> I speke of manye hundred yeres ago!
> But now can no man see none elves mo."

And alas, with the elves have gone the incubi and witches, the devils and angels, the cherubim and seraphim, until for most of us God on his sapphire throne is the solitary inhabitant of the spiritual world. And there are statistics to show that the most eminent

scientists have dismissed this concept also. Mr. Sherman himself seems to reduce God to that benevolent abstraction, "a power not ourselves that makes for righteousness."

But if the third floor front is almost vacant, there is yet "the home of eternal ideas" in the third floor back. Among these eternal verities, doubtless, are the elementary principles of conduct, which, Mr. Sherman declares in his introduction, "have been adequately tested and are now to be unequivocally accepted." When were the tests completed? In 1791, when Burke announced that no more discoveries were to be made in morality? Or within the last fifty years and one, since the *Nation* has upheld the changeless principles of idealism? Only twenty-five years ago the *Nation's* review of *Tess of the D'Urbervilles* came out flat-footed for a double standard of morality. Is this one of the tested principles of conduct to be unequivocally accepted, or has the perfected idealism been defined only within the last quarter of a century? The humanist critics, Messrs. Babbitt, More, and Sherman, seem as reluctant to ventilate their eternal ideas in categorical form as the Germans are in the matter of their war aims, and doubtless for the same reason: they would be starting a fight behind their own lines. The home of the old eternal verities is being prepared for evacuation, and we may look for the eventual passing of the third floor back.

It has always been a taunt of the humanist critics that the romantic writers lived in an ivory tower, remote from the crowd and bustle of life, and dreamed of man as he never was and never could be. Does not Mr. Sherman's analysis of human nature reveal the humanists themselves dwelling in an ivory tower of academic contemplation, the walls lined with the orthodox classics, the vaults containing the latest authoritative copy of the Decalogue for the benefit of the inmates? When they wish to discover what is the nature of man, they take from the shelf Aristotle or Shakespeare or even Hooker, and without asking awkward questions imbibe reverently the inspired conjectures which once upon a time did duty for a systematic, experimental study of the mind. If we do not look to the ivory tower of romanticism for the best that has been thought and said in the world, why should we listen below the ivory tower of humanism for echoes of the rudimentary psychology and ethics of ancient Greece and Elizabethan England!

CHARLES HEATON.

ON THE CONCEPTION OF PROBABILITY.

The desire for a mathematically and a philosophically sound introduction to the theory of probability is dictated by the importance of the field of applications, by the fact that the theory of probability is mathematics, and by the philosophical interest attached to the term probability. The applications of the theory were originally confined to problems in gambling, but they are now found in statistics, theory of error, statistical mechanics, insurance, etc. This expansion of the field of usefulness would by itself be a sufficient cause for minor changes in the fundamental conceptions and definitions, just as, for instance, in the steel industry the development in methods and use has been followed by changes in the standard definitions and specifications of iron and steel. To this comes that the introduction of new mathematical and logical methods, such as the axiomatic method, has furnished new view-points for the initial steps in the theory of probability. And the inquiry of philosophy into the nature of probability can never be expected to be answered absolutely and finally. In this way all of the relations of the theory to its applications and sources are likely to exert their influence on the fundamental part of this subject. The fact remains that it is extremely difficult to give a satisfactory definition of probability; quoting Poincaré, it is even hardly possible.[1] These conditions taken together explain why the question "what is probability?" in spite of its long history is still alive, and this serves as an apology for the reappearance of the subject in this paper.

The historical methods of introducing probability may be classified according to their relation to four principal methods. The first three of these will, for the purpose of orientation, be briefly mentioned. The discussion in connection with the fourth of the principal methods forms the main part of this paper. It presents a view-point which is thought to throw some new light on the question. The classification follows:

1. Bayes's definition based on the notion of mathematical expectation (*espérance mathématique, mathematische Erwartung*).

[1] H. Poincaré, *Calcul des probabilités*, ed. 1912, p. 24.

2. The method involving the introduction of the notion of equally possible cases as a fundamental notion.
3. The axiomatic method.
4. The method which will here be called the statistical, which has been used by statisticians, and which is based primarily on the law of great numbers.

1. Bayes's ideas of probability are perhaps the oldest historically. At least according to Von Kries's interpretation, they are underlying, though not definitely formulated, in the classical correspondence between Pascal and Fermat.[2] Bayes expressed those ideas in a definition.[3] An example will explain the principle involved. Playing head and tail with two dollars at stake, with even chance of winning and losing, the value of the chance to win is reasonably estimated as one dollar. Then, according to Bayes's definition, the probability of winning would be expressed as one dollar divided by two dollars, that is 1/2. If in general the reasonable value of the chance—the mathematical expectation—of obtaining A is B then B/A is the probability. It seems evident that the modern student of applied mathematics can hardly expect to obtain the clearest notion of probability by way of Bayes's definition unless he has occupied himself extensively with gambling. It seems that only thereby may one develop the notion of the value of a chance, the notion of the mathematical expectation of gain, as a fundamental conception or as an idea of an existing tangible reality. The definition depends on a reference to this reality. Bayes's definition though interesting has now chiefly historical value.

2. The second principal method, found in many classical discussions, makes use of the expressions equally possible or equally probable. These expressions are taken from the ordinary spoken language, and their meaning remains essentially unchanged after they have been introduced and used in the mathematical theory. Illustrative examples are used among which one is predominant, or fundamental as far as it may be said to represent schematically the formation of any probability. It is the example of the bag containing balls of different color but otherwise the same. Assume p white balls in the bag out of a total of q, then there are p equally possible cases out of q in which one draws a white ball by taking one out. Then by the definition the probability is p/q. This gen-

[2] Von Kries, *Die Prinzipien der Wahrscheinlichkeitsrechnung*, 1886, p. 267.

[3] Th. Bayes, "An Essay toward Solving a Problem in the Doctrine of Chances," *Philosoph. Transactions*, 1763, p. 370.

eral method has its disadvantages. What is "equally possible"? To answer this additional explanations of great length have been deemed necessary at the various times. It is sufficient at this place to mention the classical works by Laplace, J. F. Fries, Lexis, Von Kries, and Bertrand, and besides, two more recent discussions by Lourié and Grelling.[4] In spite of the high value of these works, in spite of the increased understanding of probability due to them, their inevitable extensiveness certainly makes them less accessible than is to be desired. And if the notion of the equally possible cases is maintained as the fundamental idea, it is not unlikely that further discussions on the same basis will be found necessary in the future.

3. The *axiomatic method* has been tried by Broggi and Bohlmann.[5] Terms such as "event A," "probability of event A," "event A independent of B or excluding B," etc., are introduced in the axioms stating the two principal laws of combining probability. The axioms can be stated briefly, and logical rigidity as far as the mathematical theory itself is concerned may thus be obtained. When partially independent notions are to be introduced, such as, for instance, that of continuity of probabilities, then the necessary additional axioms are established without difficulty.[6] Nevertheless, as mentioned by the originators of the axioms, the method solves the questions involved only in part. If one does not know what probability is before the axioms are stated the chance remains that one will not know it afterward either. The logical problem left is essentially that to which the main efforts of the previously mentioned extensive works were devoted. The axiomatic method then has its value as supplementary to other solutions. Between the axioms by themselves and reality there is no bridge.

4. The fourth and last principal method is the *statistical method*

[4] Laplace, *Théorie analytique des probabilités,* philosophical introduction from 2d ed. on. J. F. Fries, *Versuch einer Kritik der Prinzipien der Wahrscheinlichkeitsrechnung,* 1842. W. Lexis, *Zur Theorie der Massenerscheinungen,* 1877. Von Kries, *Die Prinzipien der Wahrscheinlichkeitsrechnung,* 1886. Bertrand, *Calcul des probabilités,* 1889 (introduction). S. Lourié, *Die Prinzipien der Wahrscheinlichkeitsrechnung,* 1910. K. Grelling, *Die philosophischen Grundlagen der Wahrscheinlichkeitsrechnung, Abhandlungen der Friesschen Schule,* 1910.

[5] G. Bohlmann, *Encyc. d. math. Wiss.,* Vol. I, Part II, Art. 1D 4b (1900-1904). U. Broggi, *Die Axiome der Wahrscheinlichkeitsrechnung,* Dissertation, Göttingen, 1907. G. Bohlmann, *Die Grundbegriffe der Wahrscheinlichkeitsrechnung, Atti del 4. Congresso Internazionale dei Matematici,* Roma, 6-11 Aprile 1908, Vol. III, 1909, pp. 244 etc.

[6] The point of view of the continuous probabilities is emphasized by L. Bachelier in his *Calcul des probabilités,* 1912.

based principally on the law of great numbers. Montessus by deriving his definition of "equally possible" from the experience expressed in the law of great numbers becomes a representative of this point of view.[7] The writer believes that the statistical method is best suited to the needs in the present main fields of applications, though it is realized that the completest understanding of the problem is reached by a study of all the historical methods. The discussion which follows will propose a method of obtaining logical rigidity in definitions on the basis of the law of great numbers. We shall in the first place consider only the probability which applies in the theory of probability. Other types of probability, subjective and psychological probabilities, which do not necessarily follow the same laws, will be briefly mentioned afterward.

In order to prepare the way for a definition of the probability in the the theory of probability two preliminary notions will first be introduced: that of a great probability and that of a great number.

First let us consider the expression "great probability." This expression shall first be taken in the sense in which it is used in the ordinary scientific language. It shall express the almost safe, or as good as safe expectation that a certain event will occur. Such great probabilities exist. They are derived essentially from experience, but it is realized that they also contain a subjective element expressed in the decision to believe in a certain regularity that makes predictions possible, or in the decision to disregard certain very slight possibilities as immaterial.

The other preliminary notion is that of the great number. The expression "a great number" shall first be taken in the sense of the ordinary scientific language, but in order to adapt the notion for mathematical use we add the following statement: let the functional forms

$$F_1(N_1, N_2 \ldots N_k),\ F_2(N_1 \ldots N_k), \ldots$$

represent certain uses made of the numbers $N_1, N_2 \ldots N_k$; let $n_1, n_2, \ldots n_k$ represent any k numbers less than a certain number n; then $N_1, N_2 \ldots N_k$ are said to be great numbers with respect to the use $F_1, F_2 \ldots$, and compared with the number n, when—besides $N_1 \ldots N_k$ being great numbers in the ordinary sense—the differences

$$F(N_1+n_1, N_2+n_2 \ldots) - F(N_1, N_2 \ldots)$$

can be neglected.

[7] Montessus, "La loi des grands nombres," *L'enseignement mathématique*, 1905, pp. 122-138. See also his *Calcul des probabilités*, 1908.

That "great numbers" as just defined exist is a matter of experience. When and whether the differences mentioned can be neglected is not merely a mathematical question, but it depends on the empirical realities to which they apply. The explanation "great with respect to a certain use, compared with a certain number" shall always be understood as added whenever the expression "great number" is used in the theory of probability.

After this preparation it is possible to formulate a definition of probability.

Assume that a group of conditions can be indicated under which a certain event may or may not occur. Assume that the nature of this group of conditions allows their repetition any number of times. Among the conditions will be some which limit the knowledge of what actually happens at the individual reproduction of the conditions. Denote by N_2 the unknown number of times in which the event occurs during N_1 repetitions of the conditions. Assume further that N_1 and N_2 are great numbers with respect to any possible use of the fraction N_2/N_1 as represented by

$$F(N_1, N_2) = \phi(N_2/N_1),$$

N_1 and N_2 thereby being compared with some chosen number n. Assume now that the fraction N_2/N_1 with a great probability can be declared to be equal to some distinct value p, and that this great probability can be made a still greater probability by increasing N_1. Then in the sense of the theory of probability p is the probability of the given event under the given conditions. Briefly expressed, the greatly probable ratio of frequency at a great number of repetitions of the conditions is the probability.

That such "probabilities" exist is a matter of experience. Their existence is identified with the existence of the law of great numbers.

In the philosophical introduction to his Theoretical Physics Volkmann[8] advocates what may be termed a repeated epistemological or knowledge-theoretical cyclus. The present problem allows an application of this method of thought, which, though here it may at first appear so, is not a "vicious circle" but rather a "cyclus of logical convergency."

First note that the conception of probability as defined here depends on the previously defined notion of "great probabilities." But the definition of probability just given allows to consider the great probability as that special case of the general probability in

[8] P. Volkmann, *Einführung in das Studium der theoretischen Physik*, 2d ed., 1913, pp. 349 etc.

which this becomes very nearly equal to one. By adding this consideration a sharpened definition of the term "great probability" is obtained, and again, this improvement in rigidity propagates itself into the definition of the general probability. By re-applying the same method the process of sharpening the definitions may be continued.

It is easy to derive the two fundamental theorems of combining probabilities from the definition given here. The second of the theorems, stating that the probability of the contemporary occurring of two mutually independent events is equal to the product of the probabilities of the single events, requires a special definition of the term independency; such definition can be formulated as follows: the event A is said to be independent of the event B when the ratio of cases in which A occurs at a great number of repetitions of conditions is the same whether the total number of cases or only those cases favorable for the event B are considered.

After these two fundamental theorems Bernoulli's theorem of the great numbers can be derived in the usual way. This theorem throws a new light on the definition of probability and on the notion of great numbers, and thus it opens the way for another application and reapplication of a Volkmann's epistemological cyclus.

We are now ready to discuss briefly other types of probability, namely the psychological and subjective probabilities. These are distinct from the already defined empirical or hypothetically empirical or derived hypothetically empirical probabilities treated in the theory of probability. The psychological probability is the degree of expectation. It expresses itself in certain muscular strains and might be measured through these. Our expectations are not always reasonable or logical, therefore it is evident that they are not subject to the laws of the theory of probability. A type of subjective probability may be defined parallel to the empirical probability as the subjectively expected ratio of frequency at a great number of repetitions of generating conditions. Even the so defined subjective probabilities can only approximately follow the laws of the theory of probability, unless by added axioms they are made dependent on these laws. The understanding of the subjective probability improves the knowledge of the subjective element of the "great probability," one of the terms introduced at the beginning of this development. Here again appears the advantage of the epistemological cyclus.

A final cyclus leads now from the last improved conception of probability to the theory of probability, then to the applications

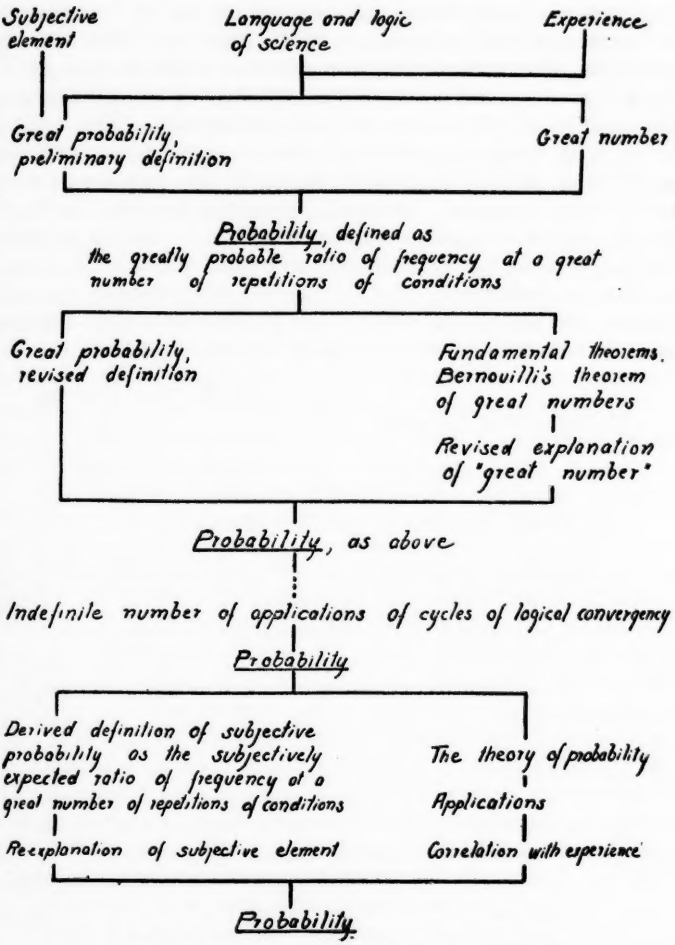
Subjective element
Language and logic of science
Experience
Great probability, preliminary definition
Great number
Probability, defined as the greatly probable ratio of frequency at a great number of repetitions of conditions
Great probability, revised definition
Fundamental theorems. Bernoulli's theorem of great numbers
Revised explanation of "great number"
Probability, as above
Indefinite number of applications of cycles of logical convergency
Probability
Derived definition of subjective probability as the subjectively expected ratio of frequency of a great number of repetitions of conditions
The theory of probability
Applications
Reexplanation of subjective element
Correlation with experience
Probability

CHART SHOWING PROCESS OF DEFINITION.

of the theory of probability, and therefrom back to the empirical foundation of the definitions.

The chart showing the process of definition reviews the individual steps in this discussion. It should be emphasized that in developing the notion of probability it is necessary to recognize the combined mathematical and empirical nature of the problem. It is conceded that mathematical points, lines, and planes are abstractions. They have neither been observed in the physical world, nor can they be visualized, and thus far, they do not exist outside the paradise of the student of pure mathematics. The relation between the abstract geometrical elements and the corresponding graphical or physical elementary objects is that they are at most mutual approximations. Successful geometry has been developed in spite of that or perhaps on account of that. Abstract probabilities might be derived by eliminating all but the merely mathematical qualities of probability. But in the field of probability the gulf between abstraction and reality is less transparent, and its bridging by proper methods of definition is of decided importance.

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RECENT WORK IN MATHEMATICAL LOGIC.

A delightful simplification of the primitive propositions required in Russell's logic (see *Principia Mathematica,* Vol. I, 1910) is made by J. G. P. Nicod in a graceful piece of work (*Proc. Camb. Phil. Soc.,* 1916, XIX, 32-41). It will be remembered that four functions of propositions are used in Russell's logic—not-p, p or q, p and q, p implies q: of these, two are taken as indefinables. Nicod makes use of Sheffer's idea (*Trans. Amer. Math. Soc.,* XIV, 481-488) using "p *stroke* q" to mean "not both p and q" and defines the four functions ordinarily used in terms of this one indefinable. The stroke may be called the sign of *incompatibility.* By means of *three* primitive propositions the propositions required for mathematical logic are developed. The primitive propositions are as follows:

1. If p and q are elementary propositions, so is p *stroke* q.

2. If p and p *stroke* (r *stroke* q) are true, then q is true.

3. P *stroke* (π *stroke* Q), where P stands for p *stroke* (q *stroke* r), Q for [s *stroke* q] *stroke* [(p *stroke* s) *stroke* (p *stroke* s)] and π for t *stroke* (t *stroke* t).

The generalized form of the principle of inference (the second

of these propositions) is interesting and deserves notice. The third of these primitive propositions is of course exceedingly complicated and does not commend itself as true so readily as the primitive propositions of *Principia Mathematica,* but it is very interesting to have found it and to have shown how the more familiar propositions can be deduced. In the same issue of the *Proc. Camb. Phil. Soc.* C. E. van Horn uses the same indefinable calling it "*p deltas q.*" Then from a certain group of three primitive propositions he claims to be able to deduce the propositions required for logic. These propositions consist of the principle of inference as given in *Principia Mathematica,* a proposition analogous to prop. 1·71 in *Principia Mathematica,* by means of which it is possible to deduce that *p deltas q* is an elementary proposition, when *p* and *q* are elementary propositions; and a third proposition stating that if *p* and *q* are of the *same* truth value, *p* and *p deltas q* are of oppositive truth values, and that if *p* and *q* are of *opposite* truth values, *p deltas q* is true. However, in the development of this system, it is clear that some axiom is required to connect "deltas" with "of the same truth value." It is not difficult to find vicious circles in some of the demonstrations (cf. the criticism of Nicod at the end of his paper, *ibid.,* p. 40).

C. I. Lewis (*Journal of Phil., Psy., and Scientific Methods,* 1917, XIV, 350-355) gives a further statement of his views on the nature of material implication (cf. *Mind,* 1912, number 84; 1914, number 90) and continues his criticism of Russell's use of the notion.

A translation of part of Frege's *Grundgesetze* is given in *The Monist* (1917, XXVII, 114-127). A. E. Heath (*ibid.,* pp. 1-56) contributes an interesting account of Grassmann's work. D. M. Wrinch (*ibid.,* pp. 83-104) gives a sketch of Bernard Bolzano's life and his pioneer work in mathematical logic. Philip E. B. Jourdain (*ibid.,* pp. 142-151) discusses existence and distinguishes the "entity" of a number from its "existence."

N. Wiener (*Trans. Amer. Math. Soc.,* 1917, XVIII, 65-72) attacks a very general problem in Boolean algebras, finding the necessary and sufficient conditions that a relation between any number of elements will remain invariant with reference to all transformations of the algebra into itself which may be expressed in the symbolism and which leave it a Boolean algebra.

An extract from Couturat's unpublished *Manuel de Logistique* is given in the *Rev. de Métaphys.* (1917, XXIV, 15-58). The

nature of propositional functions, formal and material implication, and real and apparent variables is examined. The article concludes with a sketch of the more elementary parts of the calculus of classes and the definition of 0 and 1. Part of an unfinished treatise written by Couturat sometime before 1902 is given in the same periodical (*ibid.*, pp. 291-313), which is interesting as an exposition of the *frequency* view of probability originally put forward by Venn and the mathematical writers, as opposed to the *sufficiency* view held by Bosanquet, Lotze, and Sigwart. *Probable* is said to be significantly predicable only of indeterminate judgments (by which is meant, presumably, propositional functions) and not of events or determinate judgments. The probability of a judgment is then defined to be the ratio of the number of cases when it is true to the number of cases when it is true or false. The article ends with a short account of the elementary part of the calculus of probabilities. Tenney L. Davis (*Journal of Phil., Psy., and Scientific Methods,* 1917, XIV, 421-440) gives an interesting study of the theory of probabilities, discussing some of the philosophical problems as well as the calculus.

F. Enriques (*Rev. de Métaphys.*, 1917, XXIV, 149-164), in an article on the mathematical infinite draws a distinction between a *potential* infinite and an *actual* infinite. As an illustration he gives the set

$$\cdot 3,\ \cdot 33,\ \cdot 333,\ldots\ldots\ldots$$

Now, this ordered set can be given in two ways. (1) The general term consists of so many threes and the terms are ordered by the relation "less than." (2) Each term is obtained from the term before, the first term being $\cdot 3$. In the first case we are said to have an *actual* and in the second case a *potential* infinite. The real antithesis, however, seems to be that in the second case we have the terms given as the field of a relation which is one-one, relating only consecutive terms, whereas in the first case the terms are given as the field of a relation which is transitive and symmetrical, and thus relates any early term to any later one. In his brilliant work on the *ancestral* relation (see *Begriffsschrift*, 1879, Part III, pp. 55-87, *Grundgesetze der Arithmetik*, Vol. I, 1893, §§ 45-46), Frege has shown how to manufacture a relation having the formal characteristics of a series from a one-one-relation: "less than'" is the ancestral relation obtained from the relation between terms in (2). There seems to be no fundamental logical distinction with respect

to the nature of the infinite as Enriques suggests. The question of the validity of the assumption of the existence of classes is discussed, but no mention is made of the work of Whitehead and Russell (cf. *Principia Mathematica,* Vol. I) which suggests a method of logical construction by means of which this assumption can be avoided.

A. Padoa (*Rev. de Métaphys.,* 1917, XXIV, 315-325) discusses the general problem of changing the primitive ideas in a deductive system. In an interesting article on the function of symbolism in mathematical logic (*Scientia,* 1917, XXI, 1-12) Philip E. B. Jourdain answers certain charges brought by Rignano against mathematical logic (*ibid.,* 1916, XX). He points out that, though the aim of symbolism in mathematics and logic has been until recently to make the process of reasoning mechanical by pointing out algebraic *analogies,* the aim in modern logic has been to increase the subtlety of our thought by emphasizing *differences.* An account of the work of Peano, Frege, and Russell is given.

In an article, entitled "The Organisation of Thought" in a book of the same title (London, 1917), Prof. A. N. Whitehead investigates the relation between science and logic, and describes some of the features of modern logic.

Prof. L. P. Saunders (*Mind,* 1917, number 101) in a criticism of Mr. Russell's *Lowell Lectures* discusses the nature of logical constructions. The subject is interesting to the mathematical logician, in that, on the validity of this method rests the validity of all but the most elementary parts of mathematical logic, for classes, relations, and numbers are all logical constructions. D. M. Wrinch (*ibid.,* 1917, number 104) attempts to answer some of the points brought up by Professor Saunders in his attack.

Raphael Demos (*Mind,* 1917, number 102) contributes an interesting article on particular negative propositions. He assumes that there are no negative facts and deduces that negative propositions must have reference to the world of positive facts. Particular negative propositions are said to be *ambiguous descriptions of positive propositions* and they are treated as Russell treats descriptive phrases (see *Principia Mathematica,* Vol. I). It seems, however, to the reviewer a little unwise to base a theory on such a disputable point as the non-existence of negative facts.

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MONISM AND DUALISM.

Are monism and dualism incompatible? or are they complementary aspects of existence? Is human experience such as to be satisfied by either one of these attitudes? or does it demand both?

Our point of view is determined, not immediately by being-in-itself, but by our experience of being. Therefore if man may experience being in two distinct ways he also may see being from two different points of view.

According to Plato's theory of knowledge man may know being either through sense-perception produced by the material manifestations of being, or through immediate intuition of unmanifested being. This thought reconciles monism and dualism. And the fact that it does so would seem to be good reason for accepting it.

Through sense-perception we know the material world. This world is so constituted that it generally leads us to a dualistic point of view. Only those who confine their attention to the physical side of nature come to look on the world as the manifestation of a single principle. Supersensible experience, on the other hand, may contain no duality, and therefore may lead naturally to a monistic point of view. If thus sense-perception and supersensible experience constitute two distinct spheres of experience which never combine in human consciousness, and which are separated by a gulf impassable by thought, it is quite proper that one of these spheres of experience be interpreted dualistically while the other demands a monistic point of view. The student of nature may be right in seeing the world as a manifestation of two principles. The monist, for whom perhaps the world does not exist while he sees being from the supersensible point of view, may also be right in seeing all as one.

The universe is made of forces. All these forces are probably alike. As far as we know the differences between things lie entirely in the difference of arrangement of the forces of which the things are made. The arrangement of the forces results from their local motion. When the local motion of the forces in a piece of wood is increased by throwing it into the fire, these forces cease to make a piece of wood and instead form themselves into smoke and flame and ashes. This local motion, in conjunction with the local arrange-

ment which results directly from the motion, is called energy. Each thing in the universe is a distinct thing by virtue of the energy which it contains.

It is the nature of energy not to stay where it is put, but to dissipate itself. This peculiarity of energy has led to the formulation of the Law of Dissipation of Energy. According to this law energy is never transmitted without being at the same time dissipated. We also know that our sensations result from transmissions of energy, either from things to our bodies, or from our bodies to things. No concentration of energy, however great, could in the least affect our senses so long as the energy is locked up in the thing. Energy produces sensation only when it is transmitted from one thing to another. According to the Law of Dissipation of Energy this transmission results in dissipation. Hence it follows that we are able to perceive the universe only in so far as it is going to pieces. Some things, as the sun for instance, are perceived because they are going to pieces. Other things, as for instance a stone seen by the light of the sun, are perceived because another thing is going to pieces. However, the stone also is going to pieces, although at such a slow rate that that part of its own energy which it liberates produces no sensation. Every part of the universe is unceasingly going to pieces. This is the peculiarity of the universe which enables us to perceive it and to know it, and which, therefore, makes it our universe. This is the reason why we are sometimes tempted to think that the universe never does anything but go to pieces.

But when we reflect we find that the conception of a universe which is just going to pieces throughout infinite time is repugnant to the intellect. The intellect revolts against this impressionistic attitude and boldly infers that there is also creation going on in the universe. Creation consists in concentration of energy. According to the Law of Dissipation of Energy concentration of energy cannot result from transmission of energy. Therefore we are compelled to assume that energy is created at certain times, or possibly at all times.

All mechanical process requires that the elemental forces in it remain unchanged. So long as the forces involved in a process remain unchanged no energy can be created or destroyed in this process. This is the Law of Conservation of Energy. Hence it follows that creation of energy cannot result from the mechanical

or physical process, but that there must be in the universe another kind of process in which energy is created, there must be a metaphysical process. We have thus found that while our senses are affected only by the mechanical or physical or material process our intellect demands also a metaphysical process. A mechanistic or materialistic monism can therefore never satisfy the human intellect.

Natural science has shown conclusively that all the immediately observable phenomena of the universe are mechanical. Therefore no genuine idealistic monism can explain the universe. A so-called monism in which the One is merely the owner of two active principles, one mechanical and the other ideal, is not monism. Such a standpoint is dualism in disguise. The actual world evidently arises from the interaction of two opposite principles. One of these principles is mechanical and destructive. The other principle is creative; and therefore it cannot be regarded as mechanical, but it must be regarded as intelligent. Therefore it is impossible to explain the actual world from a monistic standpoint. If our monism be mechanistic we fail to account for the creative process which is implied by the physical process and which is plainly indicated by all vital, social, and intellectual phenomena. If, on the other hand, our monism be idealistic we fail to explain the mechanism and deterioration and strife which we find everywhere about us. It is evident, therefore, that in so far as we study nature or consider any aspect of the world of sense we must adopt a dualistic attitude.

We have seen that the intelligent study of nature requires that we assume not only a mechanism but also a creative principle behind natural phenomena. Through sense-perception this creative principle can be known only in so far as it has manifested itself in nature. It is claimed that through supersensible experience further knowledge of this principle is attained. In order to be freely receptive to supersensible experience it is said the attention must be withdrawn from objects of sense and wholly centered on the supersensible. Hence, the duality of the world of sense has no bearing on our attitude toward supersensible being. In supersensible experience there is found oneness of principle. Such experience therefore leads naturally to a monistic point of view.

In supersensible experience evidently matter or mechanism does not manifest itself. The concept of matter therefore is useless in supersensible experience. Furthermore, if attention is given to the concept of matter this induces sense-experience; and sense-expe-

rience excludes supersensible experience. The first step toward supersensible experience therefore is to renounce the concept of matter. And in renouncing the concept of matter we naturally drop into a monistic attitude. Incidentally we also renounce the material world and come to regard the actuality of sense as nothingness. The actuality of sense is then superseded by another actuality —the actuality of the supersensible. Thus monism becomes the point of view of supersensible experience.

Supersensible experience may develop in two different directions. It may follow an intellectual trend and bring insight, or it may consist in practical contact with being. When supersensible experience develops along intellectual lines we call it revelation, or mysticism. Practical contact with supersensible being is practical religion.

The substantial agreement of the sacred books of different peoples and different ages indicates that these books are all to a great extent genuine records of experience. The sacred books practically agree on the fundamental points of supersensible knowledge. These books teach that being is one in principle but infinitely varied in content; that being is neither movement in space nor a process in time, i. e., not material, but eternally perfect Intelligence, Justice, Harmony, Beauty, Love, Bliss; that Being is free from all evil; that it is the absolute Good.

Supersensible experience is described as a peculiarly intimate contact with being, a contact so close that it amounts to practical identity. The Mundaka Upanishad says, "He who knows the highest Brahman, becomes even Brahman." And the Prasna adds, "Yes, O friend, he who knows it, becomes all-knowing, becomes all." Plotinus says, "To see and to have seen that Vision is reason no longer, but more than reason, and before reason and after reason, as also is that Vision which is seen. And perchance we should not speak of sight. For that which is seen—if we must needs speak of the Seer and the Seen as twain and not as one—that which is seen is not discovered by the seer nor conceived by him as a second thing, but, becoming as it were other than himself, he of himself contributeth nought, but as when one layeth center upon center he becometh God's and one with God. Wherefore this vision is hard to tell of, for how can a man tell of that as other than himself, which, when he discerned it, seemed not other but one with himself indeed? And it may be that this was not vision, but some other

manner of sight, aye, an ecstasy and a simplicity and a self-surrender, and a still passion of contact and of unison."

This statement of Plotinus calls attention to one of the chief objections raised against the monistic standpoint, namely that the ego seems to go into solution in the One. This is an illusion resulting from a confusion of the subject with the object. The object only is one. The subject remains many. Many subjects may know the object as one and thus be the One in an objective sense while remaining many subjects. The fact is that the genuine monistic attitude intensifies the actuality of the ego. The ego is never more of an actuality than when it succeeds in wiping out the world of sense, including all objective personality, and is maintaining its identity with the One. Absorption in Brahman, or Nirvana, or union with God is not a passive state imposed on the submissive ego by the One. On the contrary, in order to maintain this union the ego must make an exertion far greater than is required in the most strenuous of worldly activities. The extinction of sensible personality results in an awakening to supersensible personality. The change which takes place therefore is essentially a shifting of the scene of consciousness. The subject remains the same subject.

The initial step in all conscious action is the recognition of the idea as a superactual existence, as a form-giving power which may enter the actual world and impress its form upon it. In action we recognize the idea as a creative agent possessing the power to break into the mechanism of matter and add an element to it so that henceforth this mechanism is no longer the same, but a new and different mechanism. This recognition consists essentially in a tacit declaration that the idea is being. This turning from the actual to the ideal is the essence of morality. An act is moral in so far as it springs from a clear recognition of the idea as the creative principle in the world. This recognition implies the complementary concept of matter as the inert medium which the idea moulds into its own form, i. e., as a passive mechanism controlled by an active intelligence. The standpoint of morality, therefore, is dualistic.

When, instead of merely recognizing the idea as revealed in the world of sense, man turns in action altogether away from the world of sense and recognizes only being as revealed in supersensible experience his action becomes practical religion. The viewpoint of practical religion is therefore monistic. Religious action, like mysticism, demands entire renunciation of the world of sense

including all sensible personality. This renunciation consists in a withdrawal of attention. This withdrawal of attention is accomplished by means of active denial of the existence of the world of sense. The attention is then centered on the revealed concept of being as found in the sacred books. The results which follow from this turning from the sensible to the supersensible form a body of facts which demonstrate the truth of revelation. These facts are accessible to all who choose to make the experiment of denying the world and recognizing the All-one. Thus through religious action there is opened an unlimited field of practical intercourse with the supersensible. And through this intercourse an intimate practical acquaintance with God may be attained even by those who have never entered the state of mystic experience.

We live in a dualistic world. By rising above the world the problem of the world is solved for us.

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GALILEO AND NEWTON.

It is perhaps advisable to consider Newton's debt to Galileo both in mechanics and mathematics rather more fully than I have done in my previous articles on Newton and the principles of mechanics in this magazine. I will try to follow the thread connecting the thoughts of these men in what follows.

One of the most striking results of Galileo's *Discorsi*, published in 1638, is that a motion does not need a force to keep it up. This fact is concealed by the constant presence around us of friction and resistance, and so even Descartes had imagined that each planet is kept in motion by a vortex in a fluid which fills all space. Galileo found, in fact, that a force changes the *velocity* of a body. Thus, without a force which would deflect a planet into a curved orbit, the planet would proceed with uniform velocity along a straight line. Also, when considering the paths of projectiles, Galileo showed that the paths arise from compounding a horizontal uniform velocity and a vertical accelerated motion of falling. The resultant of two motions at right angles to one another is a motion along the diagonal of a parallelogram whose adjacent sides are along the two directions

of motion. By considering the various points of arrival for horizontal velocity and vertical acceleration of fixed magnitudes but for various periods of duration, Galileo found that all the points lie on a parabola.

Both these advances made by Galileo were apparently quite familiar to such physicists as Huygens, Wren, Hooke, and Halley. Huygens made a very important application of Galileo's ideas to motion in a circle round a force at the center of the circle. This is, for example, the case of a stone placed on a smooth horizontal table, so as to neutralize the effect of the stone's weight, and then whirled round at the end of a string of which the other end is kept still. In this case the stone is continually deflected from a tangential path by a pull along the string to the center of the circle. We know that Newton discovered quite independently in 1666 the main result of Huygens by a method which was very probably rather different from that of which Huygens published some results in 1673. Huygens noticed that, if a circle of radius r is described with uniform velocity v, any new acceleration must, since there is no change of velocity in the orbit of the body, be at right angles to the circle at the point where the moving body is. There is thus a continual acceleration due to the force acting toward the center of which the "centrifugal force" along the tangent is the accompanying "reaction," as Newton afterward called it, and this acceleration is used up in continually changing the direction of the velocity without bringing the body any nearer to the center of the circle. If no force acted between the center and the body, the body would proceed with uniform velocity v in a straight line PT touching the circle at P, and in a time t would travel a distance vt along this line. Again, if the body at P had no velocity along PT, it would, in the time t, by Galileo's formula, travel a distance $at^2/2$ toward the center, where a is the acceleration. Of course in this case we cannot imagine the force toward the center to be transmitted by a perfectly inextensible string, for unless the string were kept stretched or the time t made infinitesimally short, there would be no force along the string. The actual motion of the body is compounded of both these motions taking place at the same time, and, if we consider t as infinitesimal, the resultant path may be considered as a straight line PQ which coincides with an arc of the circle, so that Q is a point on the circle infinitely near P. The distance TQ, which is parallel to the line joining P to the center (S) represents the dis-

tance fallen through toward the center in the short time t. Since P and Q are on a circle, the lengths PT (y), TQ (x) are connected by the relation

$$y^2+(r-x)^2=r^2$$

or, what is the same thing, by the relation between y^2 and $x(2r-x)$ proved in Euclid's *Elements* (III, 36). The above relation gives, when we substitute for y and x in it from what precedes and neglect powers of t higher than the lowest,

$$v^2=ar.$$

From this we get Huygens's formula for the magnitude of the central force,

$$a=v^2/r.$$

It may be mentioned in passing that there is some analogy between this investigation and Galileo's investigation of the path of a projectile. If the projectile starts from P with a velocity v along PT and travels a distance vt in the time t, and at the same time suffers an acceleration in the direction PS by which in the time t it describes a space $gt^2/2$, it arrives, by the end of the time t, at the point Q, where

$$y^2=v^2t^2=2v^2x/g.$$

The difference lies in the fact that in this case t need not be infinitesimal. We may say that, in the former case, the curve *begins* by being a parabola, but that the direction of the force only remains parallel to its original direction at an infinitely small distance along the curve from P, where begins the motion that we are considering.

When the empirical laws of Kepler became generally known, it seems that they were combined with Huygens's theorem by Halley and possibly others as well as by Newton, and this combination gave rise to speculation on the orbits of the planets. Indeed, it seems natural to consider Galileo's parabola of projection as passing over into an ellipse or a circle when the center of attraction is brought from infinity to a finite distance. It is probable that Hooke had no more grounds for his assertions that the force of gravitation varies inversely as the square of the distance and that this law proves that the planets move in ellipses, than this plausible analogy together with the discovery—made also by Newton in 1666 and Halley at a later date—that Kepler's third law and Huygens's theorem between them imply that the force keeping a body revolving in a circle about a larger one, as is approximately the case with the planets and

the sun or the moon and the earth, is inversely as the square of the distance.

Another aspect of Galileo's works was of especial importance in influencing Newton probably through Barrow, and this was in a purely mathematical direction. Galileo, in his attempt to find the law according to which a body falls near the surface of the earth, neglecting the resistance of the air, assumed that the velocity acquired by the end of a certain time during which the body falls is proportional to the length of the time of falling. This assumption turned out to be correct, and a previous mistaken assumption that the velocity acquired is proportional to the *space* fallen through will be referred to below. Since it was easier to find out by experiment in what way the *distance* fallen through increased with the time rather than in what way the *velocity* increased with the time, Galileo deduced, from the assumption that v is proportional to t, the relation between s and t. It must be remembered that Galileo was perfectly familiar with the ideas which were expressed in the methods of indivisibles of Kepler and Cavalieri. Galileo considered, unlike his predecessors, velocities varying from point to point, and consequently saw that we could not define "*the* velocity at a point" by the ratio of the space passed over in a finite time to that time, for different lengths of time would give different results. When, however, we consider, round a certain point, a distance which is infinitely small and therefore very nearly a straight line, then for the infinitely small time in which this space is described, we may regard the increase or decrease of space as uniform. This new notion of "velocity" as the (unique) ratio of infinitesimals includes the old one as a particular case; for if the ratio of ds to dt, as we may write these infinitesimal increments in the notation subsequently introduced by Leibniz, is constant, then s is proportional to t.

Galileo represented the lengths of time by lengths on an axis of abscissæ measured from a fixed point on it, and the magnitudes of the corresponding velocities by ordinates. In this diagram, which is like the diagrams introduced into geometry by Descartes soon after Galileo's ideas were formed, except that x and y replaced t and v, Galileo's assumed proportionality of v to t is represented by a straight line through O. That is to say, if P is any point on the t-axis and PQ is the corresponding ordinate at right angles to OP, then, when P varies in position, the ends Q of all such ordinates lie on the above straight line. Now Galileo proved that the tri-

angular area OPQ represents, in units of square measure, the space (s), in units of linear measure, fallen through. Just as the velocity when variable can be measured, at any point, by ds/dt, the slope of the curve at that point, on a diagram in which times are abscissæ and spaces ordinates, so on a diagram in which times are abscissæ and velocities ordinates, the acceleration is measured by dv/dt, the slope of the curve. It can hardly be doubted that Galileo, knowing how areas of curves are found by the method of indivisibles, saw that when the acceleration is *variable*, the area of the figure OPQ, which is no longer a triangle, still represents s. It is also quite possible that Galileo saw in this way the inverse relation of the problems of tangents and quadratures: the ordinate (v) of any point on the st-diagram is given by calculating the corresponding area on the vt-diagram, while the ordinates on the vt-diagram are determined by the slopes or tangents at the corresponding points of the st-curve. Here we may mention that, in Galileo's mistaken assumption referred to above of the proportionality of v to s, triangles like OPQ on a vs-diagram do not represent s. It will be remembered from Mach's *Mechanics* that Galileo rejected this mistaken assumption on grounds which were also mistaken. In fact, if $v=ds/dt$, the integral of $v.dt$ is s, but the integral of $v.ds$ is not s unless v is always unity, so that a vs-diagram does not show that integration is the inverse of differentiation. The notions of time being the independent variable and geometrical curves being generated by motions were used by Isaac Barrow, who was certainly influenced by Galileo and possibly by Roberval. Barrow denoted the areas by "t" and "v," and in this notation and the ideas which it implied he was followed by his pupil Isaac Newton, who, in his "method of fluxions," greatly developed the suggestive ideas of Barrow, especially Barrow's clear perception of the truth that the problems of tangents and of quadratures were inverse problems.

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CURRENT PERIODICALS.

We hope that the increasing bulk of *Science Progress*, this "Quarterly Review of Scientific Thought, Work and Affairs," is the outward and visible sign of an increasing recognition of its interest

and value. The articles in the April 1918 number are of the best and by the best. The "Essay-Reviews," the as-yet-unimitated-and-own-peculiar creation of *Science Progress* (to use the hyphenated form which gave to De Morgan many a chuckle), which combine with effect the brevity of the ordinary review and the majestic *longueurs* of the quarterly, give their writers an opportunity of using the book under discussion as a peg upon which to hang a good deal that is both relevant and urgent. The section entitled "Popular Science" has not perhaps assumed its final form. If it is a matter of popularization, it seems perhaps somewhat out of place in a periodical written by men of science for their brethren and who can manage to assimilate more solid stuff than the name would imply. On the other hand, there is a place for it if it were made to serve the function of a gibbet, so that the world at large might know the kind of nonsense that is dignified in the daily press and the magazines with the honored name of science. The "Notes" have in general a peculiar fragrance of their own, and it is easy to see in them the imprint of the wayward genius—is not all genius wayward, or is the epithet "unscientific"?—who is poet, mathematician, and savant, and who has had every reason to be out in revolt against the malignant stupidity which has characterized the attitude of the governing classes in Britain to those who have been making life possible for the white man in vast territories of the Empire, and who have been doing the work that will count the most in aid of human effort in every field of nature during the next half century. The ordinary reviews are to be counted among the best of their class. All these sections are to the good, most of them are helpful, and if irony does not always bring about the revolution desired, it plays a useful part in titillating the jaded, and in giving voice to those who have been shamefully inarticulate. But of all the features in *Science Progress* the one that strikes us as the most valuable are the forty pages or so entitled "Recent Advances in Science." There is, as far as we can recollect, nothing comparable to this section in scientific literature. Other attempts to keep the student in touch with what is going on in the world of scientific research are rather in the nature of catalogues with, at most, the shortest of summaries. But the summary is really critical, that is not mechanical but selective, that, where need be, reviews past and present and casts a penetrating eye on the future—that is what *Science Progress* gives us, and is as far as we know without a

parallel. It is the exceptional value to the writer of this notice of the pages on "Recent Advances" in two of the branches dealt with month by month in this periodical, that leads him to believe that this section is of unique and permanent value.

We have left ourselves no room to deal in detail with the contents of the April number, and must content ourselves with observing that it is brimful of interest from cover to cover, and that "R. R." seems to be here, there, and everywhere, and racier than ever.

BOOK REVIEWS AND NOTES.

THE DISTRIBUTION OF ATTENTION. By *E. Neil McQueen. The British Journal of Psychology,* Monograph Supplements, No. 5. Cambridge University Press. Pp. 142. Price 5s. net.

This monograph is a thesis approved for the degree of doctor of science in the University of London, and in it the author sets out to discover whether there is any general power of distributing the attention. In dealing with the study of individual differences, a considerable number of psychological writers have assumed that individual minds may be classified according to types (phlegmatic or choleric, sensorial or muscular in reaction, etc.), the implication being that a given mind has a power of functioning in a constant fashion, without reference to the character of the particular task performed. But this opinion is dying out, and the psychologists are increasingly inclining to the view that the individual's reaction varies in nature according to the type of performance. It is the author's purpose in this monograph to dispose of the assumption in question with reference to the topic of the distribution of attention, and by the citation of experiments of his own, to prove that the distribution of attention in the case of any particular person is a variable factor and that consequently there is no general "span" of attention.

The author begins with a critical review of previous work, making mention of tachitoscopic experiments intended to measure the range of visual attention and other experiments in the field of successive auditory impressions. Reference is made to an interesting experiment by Binet in which the latter instructed his subjects to press a rubber tube according to a certain rhythm which they were to endeavor to maintain while doing something else, as for example, reading aloud, or calculating mentally; Binet believes that the number of impressions which an individual can perform on the tube while doing the other mental work will give some indication of the range of his field of consciousness. The author brings the following criticism to bear on the reasoning usually applied to this kind of experiment. First, that no method is indicated of measuring the two processes simultaneously performed, so that it is impossible to compare the span of the individual who, while recording correctly six impressions on the tube, yet takes ten seconds to perform an addition and then gets it wrong, with that of an individual who performs the addition correctly in five seconds but fails to record correctly more than two impressions on the tube. And, secondly, that no measurement is made of the individual's ability

to perform singly each of the two processes performed simultaneously during the experiment, whereas it is clear that an individual who excels in either performance done alone will also do well in the performance when done simultaneously with another. Thus, the experiments are criticised by the author as inconclusive, because they have not been carried out so as to exclude alien factors or in a way to secure precise measurement of results.

The author then relates his own experiments carried out, as he says, to obtain a measure of the ability of the subject for the "undistributed" performances (i. e., of the performances carried on singly), to eliminate the effect of these differences of ability in the undistributed performances, and to see if any evidence were forthcoming of the existence of any general factor of distribution of the attention common to all the "distributed" performances.

In the process of the experiment, the subjects perform a certain pair of tasks, both singly and simultaneously, and in each case their efficiency is measured and recorded respectively; they perform not one pair merely but a number of different pairs of tasks (tapping and adding, card-sorting and counting by 3s, etc.). With these results, the investigator was able to calculate the ability of the subject for the undistributed as well as for the double performance in each case, and, finding that there is an appreciable correlation between the undistributed performances, reasoned—correctly—that the factor which produced this correlation would operate to produce a high correlation between the corresponding distributed performances. But, he argued, if there is a general power of distribution, there must be a further factor owing to the operation of which, the correlation between the distributed performances should be higher than that between the undistributed performances.

Testing this deduction from the hypothesis of a general power of distributing the attention, with the experimental results, the author discovered that the averages for the intercorrelations of the distributed and undistributed performances were nearly equal and hence concluded that there is no evidence of a general power of distribution. From other experiments, which consisted in letting the subject perform repeatedly tasks of the same sort but with varying speed, the author ascertained that the distribution of attention varied as the speed varied, a fact supporting the view of the specific nature of the factors involved in any distribution of the attention.

In succeeding chapters the author deals with a variety of possible deductions from the results of his experimental researches and concludes by considering the vexed question whether it is possible to attend to more than one thing at a time independently. One must remember that what appears as a plurality of objects in practice, may be psychologically a single object; other alternatives in the case of the performance of two tasks simultaneously are an alternation of the attention from the one to the other, an automatization of one of the processes and its consequent relegation to the field of consciousness, and simultaneous attention to the two tasks, both being present independently in the field of attention. Introspective evidence obtained under experimental conditions is brought forward in proof of the occurrence of all these possibilities, including the last. It is rather difficult to understand how one should be able to attend simultaneously to two tasks as separate, but the author takes the sting out of his conclusion by admitting that the conscious effort, at any rate, is directed to only one of the tasks.

It has been possible to give only a brief and very inadequate summary of Mr. McQueen's account of his own experimental researches and of his criticisms of the work of others in the same field. The investigation is strictly scientific and experimental, and the author proves himself resourceful in devising experiments calculated to yield precise results. The exposition is as clear as the subject will allow, and the whole account evidences a happy combination of the empirical-experimental habit of mind with the ability for coherent thinking. The results of Mr. McQueen's investigations should inflict one more blow upon the fast crumbling structure of the old superstition which regards the mind as a substance endowed with general powers as such, instead of as a thing analogous to an organism, whose action is a function in each case of the particular situation in which it finds itself. Δ

HERBERT SPENCER. By *Hugh Elliot*. "Makers of the Nineteenth Century" series, edited by *Basil Williams*. London: Constable and Co., Ltd., 1917. Pp. x, 330. Price, 6s. net.

This volume is a well and clearly written account of all that is of any importance in the life and work of Herbert Spencer (1820-1902). Mr. Elliot is by no means thoroughly in sympathy with either the character or the doctrines of that singularly estimable and unattractive man, who was always admirable except when lack of humor made him ridiculous; although it would seem that at first Mr. Elliot was a very ardent admirer of Spencer's writings. It was certainly much the best plan to give, as Mr. Elliot has given, by far the larger part of the book to a description of the writings of Spencer, which are very numerous for one who was "by nature an exceedingly idle man" p. 61). Mr. Elliot's opinion is that the main interest of Spencer's works at the present time is on the social side. "His scientific and evolutionary writings have already become part of the 'atmosphere' of modern thought, in the sense that they scarcely need to be taught, but constitute the foundation upon which more recent ideas are built. But his social writings have not in the same way become axiomatic...."(p. 2).

There are two fundamental ideas at the root of Spencer's philosophy. The first is that of universal evolution. Early in life Spencer "endeavored to find some law which should describe the tendencies of the constant state of change in Nature,—a law which should be equally applicable to the change of a nebula into a star or stellar system, and of a protozoan animal into a man. This law he called the Law of Evolution. It proposed to describe the various stages characteristic of all progress in all departments of Nature as the universe grows older. He believed that the outlines of such changes were similar throughout all varieties of the changing substance" (p. 79). The second fundamental idea is the guiding principle of his social and political writings, and is that social progress consists in "the admission of every individual to the *maximum* freedom consistent with social order and security" (p. 80).

In the statement of the principles of his philosophy Spencer was very prone to those sonorous definitions which seem to impress some people as evidences of great power and profundity. With their help Spencer brought about an apparent transformation of sciences into a form in which practically everything that seemed, to those people who combine interest in science with

lack of scientific ability, to matter, could be logically deduced from them. Examples of such verbiage are to be found in the quotations on pages 244 and 254, for example. However, on a closer logical inspection it can be seen without much trouble that the part played by strict deduction in Spencer's work is usually an illusion. Such a mathematician as Laplace never fell into the Spencerian errors in framing his nebular hypothesis, and consequently Laplace's theory certainly looks, at first sight, less complete than Spencer's. But Spencer only arrived at apparent completeness owing to mathematical ignorance on his own and his readers' parts, and by unhesitatingly taking steps which no trained logician or mathematician would venture to call "deduction." Also no mathematician would do such a ludicrous thing as to "deduce" the motion of more than one body from the principle of the conservation of energy; and yet Spencer does something very much like this when he makes certain conclusions follow from the "Persistence of Force." Thus we cannot agree with Mr. Elliot when he says (p. 57) that Spencer's "philosophy is in many respects Euclidean in form...," unless of course the phrase is meant to be taken in a vague and popular sense, much as some people say of a child who does not cry when he is hurt that he is a "philosopher." The interesting remarks on Spencer's capacity for generalization (p. 62) seem, then, to lay stress on the point that often Spencer was led astray by what he fancied to be analogies.

It is hardly worth while nowadays to insist on the failure of Spencer's attempts to write about metaphysics. Mr. Elliot has said a few words on this subject on pp. 216ff, with most of which it is impossible not to agree. On this subject Spencer's remark, of which he actually seemed proud, that he could not read Kant because he saw he did not agree with him, is rather relevant.

There is a very good portrait of Spencer prefixed to this volume, after the painting in the National Portrait Gallery by J. B. Burgess. ϕ

A Philosophical System of Theistic Idealism. By *James Lindsay, D.D., M.A., B.Sc., F.R.S.E.*, etc. Edinburgh and London: Wm. Blackwood and Sons, 1917. Pp. xi, 530. Price 12s. 6d. net.

Dr. Lindsay claims that his work forms a system of Theistic Idealism, and that the system is intelligible, self-consistent and contravened by no known fact. The book is critical and constructive; its criticism extends to thinkers British, American, German, French, and Italian. Its contents are: Chapter I, "Foundations of Idealism: Laws and Logic of Psychology"; Chapter II, "The God of Theistic Idealism"; III, "The Metaphysics of Creation"; IV, "The Metaphysics of Time and of Eternity"; V, "History and Providence in Theistic Idealism"; VI, "The Philosophy of Nature"; VII, "The Philosophy of Science"; VIII, "The Philosophy of Art"; IX, "Freedom in Theistic Idealism"; X, "The Moral Order, and the Spiritual World, in Theistic Idealism"; XI, "Immortality in Theistic Idealism." Also, a very full Index—twenty-eight columns—of authors and subjects.

The work falls, according to its author, under universal philosophy, and makes appeal to all the philosophical disciplines. It may be of especial importance to note, for the present Journal, that he strongly stands for spritual monism, in which the human consciousness is not simply merged or absorbed in the Divine consciousness, or treated as a mere "part" of it, but is brought

into voluntary ethical union with it. The various current philosophical theories are critically dealt with, from the standpoint of Theistic Idealism. The scope and nature of the work cannot be fully described in this place, but must be gathered from study of the work itself. λ

THE ASCENT OF OLYMPUS. By *Rendel Harris.* Manchester: The University Press; New York and London: Longmans, Green & Co., 1917. Pp. vii, 140. Price, 5s. net. [Separately printed: THE ORIGIN OF THE CULT OF DIONYSUS, THE ORIGIN OF THE CULT OF APOLLO, THE ORIGIN OF THE CULT OF ARTEMIS, THE ORIGIN OF THE CULT OF APHRODITE. 1s. net each.]

Dr. Rendel Harris is an expert in that dizzy art of derivation and interpretation to which, in recent studies of comparative religion, we have become accustomed. He has every qualification of scholarship, ingenuity, and plausibility, and he makes his detective work exceedingly interesting. We are familiar with the evolution of the gods out of snakes; Dr. Harris now evolves them out of plants—plants associated, chiefly, with the thunderbolt. The oak is the thunder-tree, being struck more often than any other. Athene is born from the head of Zeus; she is also the owl, i. e., sprung from the hollow tree. From a similar association with riven trees, bees and honey acquire sanctity. "The animistic belief makes everything that thunder touches into thunder." Not only the oak, therefore, but mistletoe and ivy, which cling to the oak, are thunder. And "when the phytomorph becomes the anthropomorph, the name of the new (subordinate) thunder-deity is Dionysus" (p. 5). (The lightning-smitten Semele, his mother, is nothing but the tree.) The association of Dionysus with wine follows naturally: the sacred ivy-leaves were chewed and eaten by the worshipers. Smilax and grape-vines were trained on trees. The goat and fawn, which feed on these plants, became cult-animals. The appearance of an androgynous Dionysus is due to his identification with *both* fire-sticks, the "male" and the "female"; and the fire-sticks were made out of ivy because there is thunder in the ivy. Dr. Harris thinks that the Vedic Soma may have been a surrogate for a more primitive sacred mastication, analogous to the ivy-leaves chewed by Mænads, and suggests a similar source for the custom of drinking ivy beer on Ascension Day at Lincoln College, Oxford.

Apollo is likewise a thunder-god, the laurel being substituted for the oak, and Apollo owes his healing art to his connection with the mistletoe, a plant of supposed medicinal virtues. Artemis is identified with the mugwort, and Aphrodite with the mandrake or love-apple.

If Dr. Harris is ever found to be wrong, it will be because he clings tenaciously to a single (vegetable) line of descent. This line is certainly traceable, but it seems possible that a developed god may have an extremely complex parentage, not to be reduced to a single root. η